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What About Friction?

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In Part I of this study the reasons for accepting the validity of the "Classical Laws" of friction were given, the sources quoted, and the laws stated as follows:

1. Frictional force is directly proportional to load, that is, to the total force which acts normal to the sliding surface.
2. Frictional force for a constant load is independent of the area of contact.
3. Frictional force is independent of the velocity of sliding.
4. Frictional force depends upon the nature of the materials in contact.

These laws are accepted by one school of modern thought.

PART II

Friction As the Result of Molecular Forces

5. Friction Independent of Roughness

THE "classical laws" of friction (Sec. 1), expressed in our current text books in the same terms as those of a hundred and fifty years ago, must be considered to have acquired their validity from the thorough work of Morin (1834). Nevertheless, the mechanism of a phenomenon as apparently simple as the operation of a brake has been a bone of contention among qualified observers for an even longer period and is still a controversial question.

The theory that sliding friction is caused by the meshing of surface irregularities, as set forth by Amontons and Coulomb (Sec. 2), must be modified, if not abandoned, in the light of modern experiments, such as the following, which indicate that friction is largely independent of surface finish:

1. The friction of ground glass is *less* than that of plate glass or of glass polished to an "optical face."[1]
2. The coefficient of friction usually has the same value whether a rider is slid over a freshly prepared surface or in the same visible groove of previous trips.[2]
3. The effect of a spherical contact in bringing about the deceleration of a flywheel leads to the conclusion that friction is independent of surface polish except when the rider is of relatively soft material.[3]
4. The results of more than 1000 friction measurements on each of five different metals were essentially the same whether

[1] W. B. Hardy, "The theory of lubrication," *Report of British Assoc. Adv. Sci.*, p. 185 (1922).

[2] T. P. Hughes and G. Whittingham, "Influence of surface films on the dry and lubricated sliding of metals," *Trans. Faraday Soc.* **38**, 9 (1942).

[3] W. B. Beare and F. P. Bowden, "Physical properties of surfaces—I—kinetic friction," *Phil. Trans. Roy. Soc.* **A234**, 329 (1935).

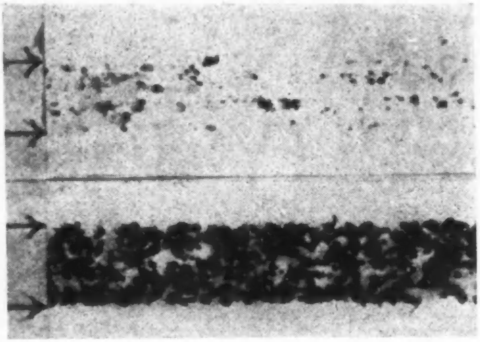

FIG. 2. Copper Track. Upper: Lubricated. Lower: Dry (Bowden and Moore, reference 13).

the surfaces were polished, or rough and torn.[4]

5. Lower frictional resistance and less frictional wear occur in bearings and cylinders in which one of the sliding surfaces is rougher than the other.[5]
6. Steel journals, with various degrees of polish, turning in lubricated bearings, show no significant differences in frictional torque.[6]

6. *Area of Contact*

Consideration of such observations leads to the conclusion that Coulomb's theory of meshing asperities can provide an adequate explanation of only one feature of frictional phenomena; hence the importance of the role played by the forces of molecular attraction should be examined more critically. These forces become effective whenever two surfaces approach each other very closely, that is, at the points of *actual* contact, and the number of these points is proportional to the area of contact between the surfaces. Hence we should expect that molecular forces should produce frictional resistance that is proportional to the area of contact—exactly the opposite of Law 2, (Sec. 3). This raises a question as to the precise meaning of the term "area of contact." Should the term more properly be applied to the actual, or touching, contact instead of the apparent, or covering, contact?

That the area of actual contact is the significant feature of the term "area," as it enters into frictional phenomena, has been generally recognized since it was announced by H. Shaw in his Cantor Lectures on Friction in 1886 (as cited by Archbutt and Deeley (1912)). He suggests that in the case of dry solid friction, where the surfaces are smooth and worn, there are even then only a certain very limited number of points of contact, and that further wear merely removes groups of particles leaving other hollows, or, if no wear takes place, merely displaces the projecting particles. Any increase of normal force brings a larger number of particles into contact, and so increases, to a proportional extent, the force of friction or the amount of rubbing and consequent heat produced. Hence the force of friction varies with the normal force.

A golfer who walks on a cement pavement in hobnailed shoes becomes painfully conscious of the fact that the area of actual contact of his hobnails with the pavement is only a fraction of the area that keeps him from slipping when he walks on the sod. True, his tendency to slip on the sod is lessened also by the fact that the hobnails dig into the sod. Similarly, when one solid surface slides over another the frictional resistance may come from two processes like the above, namely, the deformation of the points of actual contact and the plowing of furrows in one surface by the points of contact in the other.

7. *Molecular Theories*

(*a*). Some excellent work by Bowden[7–11] and his collaborators has led to conclusions that can

[4] H. Ernst and Merchant, "Surface friction of clean metals," *Proc. Spec. Summer Conf. on Friction and Surface Finish*, Mass. Inst. Tech., Cambridge, pp. 76–101 (1940).

[5] J. O. Almen, Remarks—*Proc. Spec. Summer Conf. on Friction and Surface Finish*, Mass. Inst. Tech., Cambridge, pp. 203–205 (1940).

[6] J. T. Burwell, J. Kay, D. W. Neymegen and D. A. Morgan, "Effects of surface finish," *Journ. App. Mech., Trans. A.S.M.E.* **63**, A-49 (1941).

[7] F. P. Bowden and D. Tabor, "The area of contact between stationary and between moving surfaces," *Proc. Roy. Soc.* **A169**, 391 (1938–39).

[8] F. P. Bowden and D. Tabor, "Friction and lubrication," Report No. 1; *Coun. Sci. and Ind. Res., Australia, Bull.* No. 146 (1942).

[9] F. P. Bowden and D. Tabor, "Friction and lubrication," Report No. 2, *Coun. Sci. and Ind. Res., Australia, Bull.* No. 155 (1942).

[10] F. P. Bowden and D. Tabor, "The lubrication by thin metallic films and action of bearing metals," *J. App. Physics* **14**, 141–151 (1943).

[11] F. P. Bowden, J. W. Moore, and D. Tabor, "Plowing and adhesion of sliding metals," *J. App. Physics* **14**, 80 (1943).

be summarized as follows:

1. The area of actual contact can be measured by employing a simple electrical technique (conductivity).
2. The area of actual contact of either moving or stationary surfaces is essentially unaffected by both the shape and the area of apparent contact of these surfaces.
3. Under the same load, the area of actual contact of the same metals, whether rough filed or finely polished, is the same.
4. At least in the case of metals, frictional force is proportional to the area of actual contact, which is a small fraction of the area of apparent contact.
5. Frictional force is *independent* of applied load. The principal effect of an increased load is an increased area of actual contact.
6. Deformation of metals, due to the load, is chiefly plastic. This deformation increases until the area of actual contact is sufficient to support the load; hence this area is the same whether the surface finish is rough or smooth.
7. Since the area of actual contact is very small, even a light load may produce a pressure high enough to cause adhesion (welding) of the points of contact.
8. Frictional resistance of metals is due primarily to the shearing of welds at points of contact, and secondarily, to the work of plowing the harder metal through the softer.

To one who is accustomed to regard Law 1 (frictional force proportional to normal load) as the basic law of friction, the idea that frictional force is in reality independent of normal load comes as a rude shock. The fact that an increment in load causes an exactly proportionate increment in frictional force is such a universal experience that the existence of a one-to-one relationship between load and friction is not even to be questioned. Yet here the reader is being urged to deny the validity of his own experience and to believe that no such relationship exists. Preposterous! But let us examine the situation a little more critically.

Normal load and frictional resistance are two forces whose lines of action are exactly at right angles to each other. From our study of mechanics we know that neither has any component parallel to the other, hence they are entirely independent of each other. Or, at least, they are unless there is some intermediate mechanism, such as a right-angle lever, that enables a force exerted, for instance, horizontally at one point on the mechanism to become operative as a vertical force at some other point on the mechanism, and *vice versa*.

If we adopt Coulomb's theory of asperities, just such an intermediate mechanism comes to light in the inclined planes which form the sides of the asperities. In moving horizontally the slider must also be lifted vertically in order to surmount the asperities, hence work is done against the normal load. If this load is doubled, twice as much work is called for, and this can be supplied only by doubling the horizontal force.

If we adopt Bowden's theory of molecular forces, there appears a different intermediate mechanism. It is the local welding that occurs at the points of actual contact. If the normal load is doubled, the number of contact points and hence the number of welds is doubled. If the slider is to

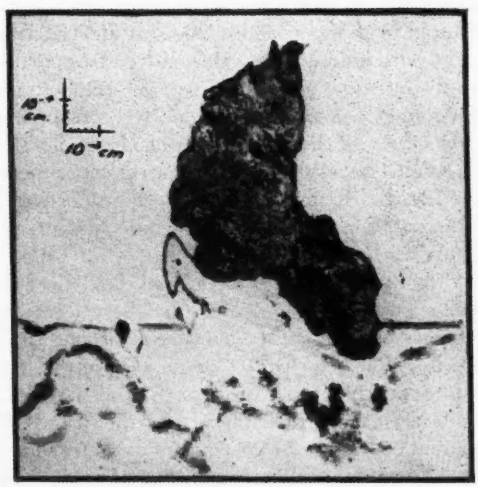

FIG. 3. Tapered section of a portion of track formed by copper on steel. This shows the welding of the copper onto the steel, and the way in which the steel has been distorted and plucked out by the copper. Horizontal magnification 50. Vertical magnification 500 (Bowden, Moore and Tabor, reference 11).

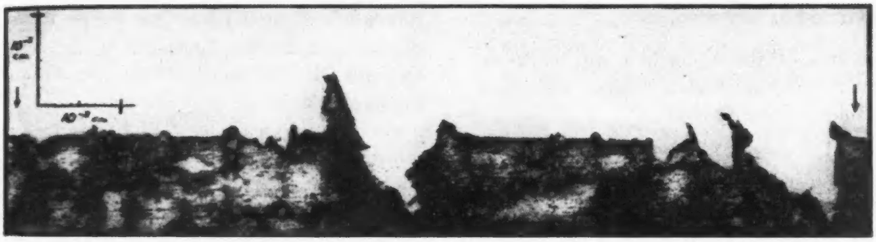

FIG. 4. Tapered section of track formed by sliding a hemispherical steel rider on an unlubricated steel surface. Original photograph has a horizontal magnification of 200 and a vertical magnification of 2000. The width of the track is indicated by the arrows. The tearing and plucking of the steel are clearly seen. The rider passed over the surface only once. (Bowden, Moore and Tabor, reference 11).

move horizontally, the applied force must be great enough to shear off the welds. Therefore, if their number is doubled, twice as much force will be required. Thus, again, there is one-to-one correspondence between load and horizontal force.

However, suppose it were possible to devise an experiment in which the contact area can be increased without increasing the load, and *vice versa*. To which factor, load or area, will the friction force be found to be proportional? Exactly this has been done most ingeniously by Bowden and his collaborators. They separated the effects of load and area by the use of sliders of different shapes. They also measured the force required to slide a steel ball along a steel surface plated with indium. Thus the load was supported mainly by the underlying steel, yet the contact area could be varied independently by changing the thickness of the indium coating into which the ball sank. The results of both types of experiment showed that frictional force is indeed proportional to actual contact area, and independent of normal load.

If any doubt arises as to the reality of discrete contact points and of local welds where shearing occurs, it should be dispelled by considering that (*a*) when polished surfaces of glass or quartz are rubbed together minute hot spots, or sparkles, may be seen thinly distributed over the surface of apparent contact,[12] (*b*) the track of a copper slider (Fig. 2) made visible by means of a special chemical technique, shows not only that points of actual contact are more numerous with a slider that is dry, rather than lubricated, but also that the difference between the two tracks indicates that the force required to shear local welds must be a major component of the friction between sliding metals,[13] and (*c*) a soft metal, such as copper, may sometimes become welded to a hard metal, such as steel, so that when sliding (dry) at low speed it plucks up the steel,[13] as illustrated in Fig. 3 (the specimen was cut at an acute angle). If sliding were continued the copper would tear away a bit of steel leaving a pit. By similar action, a hard steel shaft may be slowly worn by the softer metal of its bearing.

Photographic evidence[13] that plowing occurs, at least under certain conditions, is provided by Fig. 4, taken by the same technique as Fig. 3.

(*b*). The dependence of friction upon bulk properties was also investigated by Bowden. If, as evidence just given appears to indicate, friction involves the displacement of material below the immediate surface, we should expect the magnitude of the frictional force to be a function of the bulk properties of the material. Such a relationship has been derived for metals. The total frictional force is made up of a shearing force plus a plowing force, and the normal load equals the flow pressure times the actual area of contact. Here flow pressure is that pressure which causes plastic flow of the softer metal. If the plowing force is negligible, $\mu =$ (shear strength)/(flow pressure), where μ is the coefficient of sliding friction for the softer metal. Under these conditions the coefficient of friction should be independent of load, and should depend only upon the bulk properties of the softer metal.

(*c*). Fifteen years before Bowden began the

[12] F. P. Bowden and M. A. Stone, "Visible hot spots on sliding surfaces," *Experientia* **2**, 186 (1946).

[13] F. P. Bowden and A. J. W. Moore, "Adhesion of lubricated metals," *Nature* **155**, 451 (1945).

work described above, Hardy[14] laid before the British Association for the Advancement of Science some of the conclusions to be reached from looking at friction as a phenomenon caused by the molecular forces of cohesion and adhesion. Some of these conclusions may be described as follows:

1. Why should the frictional resistance not be proportional to the area, as it is in the internal friction of homogeneous fluids and in the surface friction of a solid face moving through a fluid? (As shown above, the frictional resistance *is* proportional to the area of actual contact—F.P.)
2. An external tangential force tends to displace and rotate the molecules at the interface between two masses of matter. Frictional force is the sum of the resistances to translation and rotation offered by all the molecules about the interface.
3. The amount of energy dissipated is the product of the force and the distance moved, and the cause of its dissipation is the vibration set up when the molecules are released from their positions of strain.
4. "If cohesion be the source of friction, why is so little resistance experienced in detaching one face from another by motion along the normal?" The answer supplied is that . . . "at a free surface of a solid the condition of minimal potential of the surface energy involves an orientation of the molecules of the surface layer such that their major attractions are in the plane of the surface. Any reaction that causes a rotation of the molecules will increase the force of attraction along the normal and so increase cohesion across the interface."
5. A film on a solid surface whether of lubricant, oxide, or adsorbed molecules, lessens its capacity for cohesion by modifying its attractive forces.
6. The striking effect of an invisible layer of oil upon friction disproves the view that friction is due to gross asperities (see Part I).

[14] F. P. Bowden and T. P. Hughes, "The friction of clean metals and the influence of adsorbed gases: the temperature coefficient of friction," *Proc. Roy. Soc.* **A172**, 280 (1939).

The problem of displacement in a normal direction, stated above in item (4), has puzzled many investigators and Hardy's answer is a very interesting one. However, the assumptions he makes seem to be unnecessary both in view of the large increase of μ with specimens treated in vacuum and in other gases[15] (discussed further in Part III), and of the large value of the adhesion between two "optical flats" (45 atmospheres, "though the real figure is probably a large multiple of this").[15]

(*d*). Tomlinson's[16] theory recognizes that the field of molecular attraction probably extends to a distance of several diameters from the center of the molecule, and that there is a repulsive force with a field which extends to a much shorter distance. This force is the one called into play when two bodies collide but are prevented from penetrating one another. Tomlinson assumes that when molecules come into contact (that is, into this repulsive field) and then separate, there is a loss of energy which is manifest as friction; hence, when atoms pass other atoms, either kind may be pulled out of equilibrium positions and cause heat.

On this theory only those atoms which help to support the load pass through the irreversible stage and so involve friction. Since there must always be a sufficient number of these in the

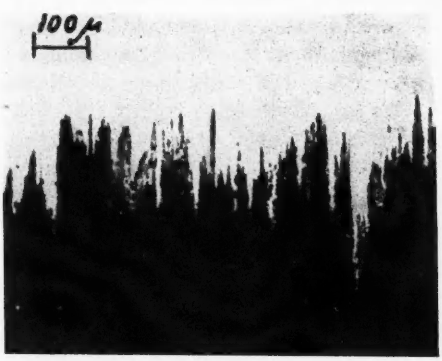

Fig. 5. Tapered section of rough ground chromium-steel flat, S.A.E. 52,100. Before friction test. Compare surface with Fig. 6. Profilometer reading 46 microinches. Magnification of original, 100 in horizontal, 2500 in vertical direction. (Sakmann, reference 18).

[15] Lord Rayleigh, "A study of glass surfaces in optical contact," *Proc. Roy. Soc.* **A156**, 326–349 (1936).

[16] G. A. Tomlinson, "Molecular theory of friction," *Phil. Mag.* (7) **7**, 907 (1929).

repulsive range to support the load, friction is proportional to this number and hence to the load. There is thus a definite chance that displaced atoms in this range may attach themselves to either sliding surface (see Sec. 8).

This theory suggests also that the coefficient of friction should be related to the elastic constants of the materials involved (compare with the theory of Bowden, Moore, and Tabor, Sec. 7(a)). Tomlinson obtained such an expression for μ containing the modulus of rigidity and the modulus of compressibility for each of the sliding materials. It is also observed that for any two materials, a and b, μ_{ab} always lies between μ_{aa} and μ_{bb}.

8. Transfer of Material from One Surface to the Other

It has already been seen (Figs. 2–4) that in the case of either dry or lubricated surfaces, materials may be transferred from the softer to the harder surface. It is not so readily shown that some of the harder material is also transferred to the softer surface. The most sensitive method of detecting a transfer of matter in either direction involves the modern technique of artificial radioactivity. A base surface consisting of a copper alloy disk, made radioactive by deuteron bombardment in a cyclotron, is rotated, and inert spherical specimens are brought into contact with it and then tested for acquired radioactivity.[17] The method enables 10^{-4} micrograms of radioactive metal to be detected. The amount of activated material (copper alloy) that adheres to the rider is proportional to the load and increases with the distance travelled. When the rider is *harder* than the base, the amount of material transferred to the rider (1) increases with roughness, and (2) with steel riders, is proportional to the area of contact. Matter is found to be transferred even for the smallest loads (with or without lubrication), that is, when the riders merely rest upon the base and then are removed, though these tests give results which are somewhat erratic.

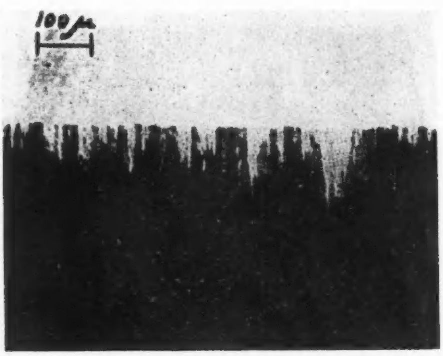

FIG. 6. Tapered section of same flat as Fig. 5, after friction test. Compare surface with Fig. 5. Profilometer reading, 37 microinches. Magnification of original 100 in horizontal, 2500 in vertical direction. (Sakmann, reference 18).

This extraordinarily sensitive method of measuring amounts of transferred material otherwise undetectable has, as yet, not been utilized to full advantage. Many more tests should be made with materials of different kinds, different hardnesses and different melting points in order to clarify the discordant results thus far obtained under conditions supposedly similar.

The transfer of material from axle to journal, and *vice versa*, is a familiar phenomenon to mechanical engineers. This "wearing-in" effect, in which rough surfaces are flattened and smooth surfaces are roughened, is well shown in the remarkable photographs[18] of Figs. 5 and 6 taken by the same technique as that used for Figs. 3 and 4.[18]

Although the operation involved in Figs. 5 and 6 is called a "friction test" it is probably more properly described as a "wear test," and unfortunately it has been shown by many observers that friction and wear do not depend upon the same properties of the rubbing materials. However, the fact that "smooth surfaces are roughened" in such a test necessitates the adoption of a theory of friction that depends primarily upon the operation of molecular forces rather than upon the intermeshing of surface roughnesses.

9. Dependence of Friction upon Velocity

In Law 3 (Sec. 1) it was stated that friction is independent of the velocity of sliding—a state-

[17] B. W. Sakmann, J. T. Burwell, and J. W. Irvine, Jr., "Measurement of the adhesion component in friction by means of radioactive indicators," *J. App. Physics*, **15**, 459 (1944).

[18] B. W. Sakmann, "Geometrical and metallurgical changes in steel surfaces under conditions of boundary lubrication," *J. App. Mech.* **14**, A-43 (1947).

ment to which authorities have clung like grim death for a hundred and fifty years from the time of Coulomb (1785). Even today our textbooks make this incorrect statement, perhaps because their authors have not had time to sift the mass of conflicting opinion on this point and determine how Law 3 should be restated more in accordance with the facts.

Yet a trip to town on the local train, if the observer keeps his eyes open, should prove that for speeds above a few feet per second the coefficient of friction increases as the speed decreases, since the engineer must ease off his brakes, set at high speed, in order to bring the train to a stop without skidding and without too much of a jerk for the comfort of his passengers. Attention was first called to this phenomenon by Galton[19] (1878), who found, during tests with railway brakes, that the coefficient of friction decreases with increase in speed, at least up to the limit of his tests (88 ft/sec). Every driver of an automobile must be familiar with the same phenomenon as he brings his car to a stop at a traffic light.

The chaos of conflicting opinion concerning the dependence of frictional force upon speed can be given a semblance of order if the results are grouped according to range of speed, as follows:

1. At very low speeds, frictional force increases with speed.
2. At medium speeds (1 in./sec to a few ft/sec), frictional force is nearly independent of speed.
3. At high speeds, frictional force decreases with speed.

If frictional force varies with speed in any such manner as this, there is small wonder that so much confusion has arisen, especially since the most convenient range for work is the one in which frictional force is practically independent of speed. In the high speed range many observers, even from the time of Coulomb, have noted a decrease in the coefficient of friction with an increase in speed. Careful experimental results at very low speeds,[20] however, are very difficult to obtain since the surfaces must be *absolutely clean*, and this condition is very difficult to attain. With

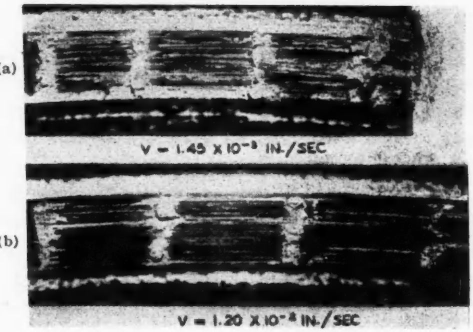

FIG. 7. Stick-slip tracks. (Dokos, reference 24).

such thorough cleansing it is also found that static friction is equal to kinetic friction—a very important conclusion, if it is found to be true in general.

At the lower end of the high speed range it is possible to express the relationship between coefficient of friction and speed by means of a formula,[21] thus:

$$\mu = \frac{1+0.0112v}{1+0.06v}\mu_0$$

where μ_0 is the coefficient of static friction, and v is the velocity of sliding in meters per second; but measurements have not indicated whether this relation is valid at higher speeds, especially since under such conditions there is softening or even melting of the surfaces which may lower the coefficient of friction markedly.[22]

It is thought that the increase in speed in the high range may lower the frictional force by causing the surfaces to separate from each other by a slight amount thus reducing the average of the molecular forces of attraction. Such separations, amounting to a few wavelengths of light, have been observed directly by several experimenters.[23]

[19] D. Galton, "Railway brakes," *Engin.* **26**, 143 (1878).
[20] C. Jacob, "Über gleitende Reibung," *Ann. d. Physik* **38**, 126 (1912).

[21] P. P. Ewald, T. Poschl, and L. Prandtl, "The physics of solids and fluids," (Blackie and Son, London, 1930) Chap. II.
[22] E. L. Armi, J. L. Johnson, R. C. Machler, and N. E. Polster, "Application of the sliding thermocouple method to the determination of temperature at the interface of a moving bullet and a gun barrel," *J. App. Physics* **18**, 88–94 (1947).
[23] J. S. Rankin, "The elastic range of friction," *Phil. Mag.* (7) **2**, 806 (1926).

10. *Stick-Slip*

Although this appropriately-named form of sliding occurs only within a limited, though easily attained, range of speed and is of everyday occurrence, it is hard to understand and more difficult to explain. Perhaps there would be no such phenomenon, if it were not for the lowering of frictional force with speed, described in Sec. 9.

1. Did you ever tease your school teacher by the squeaks of your slate-pencil moved in a nearly normal position across the slate?
2. Did you ever hear the squeaks of a door, gate or blind rotated upon its hinges?
3. Did you ever shudder at that awful noise made by an automobile as it skids to a sudden stop?
4. Were you ever bothered by the "chatter" of tools in a lathe?

The phenomenon of stick-slip which some authorities prefer to call "relaxation vibrations" might be illustrated by other examples such as the oscillations of a stroked violin string. In the latter case the string sticks to the rosin on the bow and is drawn to one side until the restoring force becomes great enough to rupture the rosin contact; then the string slips rapidly to its opposite extreme position where it is again picked up by the rosin (as described by Jeans). Any point on the string thus travels slowly to one side and then slips back very quickly. Its motion may be represented by a saw-toothed track in which the rising part is long and the falling portion very short.

The operation of stick-slip when one metal surface slides over another at a speed which is in the proper range, has been described essentially as follows:[11] The area of actual contact is very small irrespective of the load, hence pressure and heat are both concentrated in small areas rather than dispersed over the apparent contact area. The local result is high load and high temperature. If sliding takes place under these conditions the combination of high pressure and high local temperature forms a weld which holds and prevents further sliding until there is a force sufficient to break it and so to jerk one surface over the other very quickly. Thus heat is generated and the cycle is repeated.

Evidence for the periodic welding theory of stick-slip comes from microscopic examination of the track, as shown[24] in Fig. 7. The appearance of these tracks is explained thus:

> "While the surfaces are at rest relative to each other, plastic deformation occurs which allows the macroscopic irregularities of the surfaces to flow and interlock. This welding and adhesion produces larger coefficients of friction than would be obtained under the same conditions of load and surface smoothness at higher velocities."

Observation of Fig. 7 discloses that the length of slip is greater in (*b*) where the speed is an order of magnitude greater than in (*a*).

It is also shown that, with the apparatus used in these experiments, when the load is increased from 50 lb to 250 lb and the velocity is increased from 10^{-4} to 1 inch per second, the resulting frequency of stick-slip increases to about 150 vibrations per second. This is in the audible range. These values of stick-slip frequency are the same for all loads and are characteristic of the apparatus used, the apparatus must have a natural frequency upon which that of stick-slip depends. When this frequency exceeds a certain critical value stick-slip ceases. As the velocity approaches the speed at which the frequency is critical the fluctuations take the form of simple harmonic, instead of relaxation, vibrations and above the critical speed sliding proceeds smoothly.

In other apparatus[25] a squeak was emitted when a flat steel plate was moved uniformly under a curved metal rider. The squeak was of the same frequency as the fluctuation of the electrical conductance between the sliding surfaces, and was "roughly equal" to the natural frequency of vibration of the shaft to which the rider was attached. Stick-slip depends upon a dissimilarity of metals and does not occur if the metals are identical and homogeneous, though violent irregular jerking does. Stick-slip seems to be a phenomenon inherent in the process of sliding, even though the surfaces are lubricated. The mechanical properties of the sliding parts apparently influence the nature of sliding both in the laboratory and on the road; as when a tire skids silently or with a stick-slip screech de-

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[24] S. J. Dokos, "Sliding friction under extreme pressures," *Journ. App. Mech.* **13**, A-148, A-156 (1946).

[25] F. P. Bowden, L. Leben, and D. Tabor, "The sliding of metals, frictional fluctuations, and vibrations of moving parts," *Engin.* **168**, 214 (1939).

pending upon whether the speed is such as to produce a stick-slip frequency near one of the critical frequencies of the tire surface or not. While the frequency of stick-slip is often in the audible range, it may well be either subsonic or ultrasonic, depending upon the natural frequency of some vibrating part.

Other observers,[26–28] take sharp exception to the weld theory and instead ascribe stick-slip to the fact that static frictional force is greater than kinetic and the latter decreases with increase in speed. Their investigations show that whenever the frictional force decreases with increase in speed there will occur "jerky movements" like relaxation oscillations, and that the rapid fluctuations of the frictional force are the *result* and not the cause of the jerky character of the motion.

[26] N. L. Kaidanovsky and S. E. Haikin, "Mechanical relaxational oscillations," *J. Tech. Phys.* **3**, 91–107 (1933) (Russian).

[27] S. E. Haikin, L. P. Lissovsky, and A. E. Solomonovich, "On dry friction forces," *Comptes Rendus Acad. Sci. USSR (Doklady)* **24**, 135 (1939).

[28] J. R. Bristow, "Kinetic boundary friction," *Proc. Roy. Soc.* **A189**, 88–102 (1947).

11. *Summary of Part II*

It seems necessary to abandon Coulomb's theory of intermeshing roughnesses in favor of some form of molecular theory. Surface frictional behavior can then be interpreted in terms of the bulk properties of materials. Frictional forces are found in many cases to depend upon velocity, in particular to decrease as velocity of sliding increases. This is a property of those materials which sometimes slide with a jerky motion and execute relaxation oscillations or stick-slip. There may be other causes of this phenomenon.

If the student now writes down the "Laws" of friction as revised by the evidence presented in Part II, these will appear somewhat as follows:

1. Frictional force is independent of load.
2. Frictional force is directly proportional to the area of actual contact.
3. Frictional force depends upon the velocity of sliding and in the higher range decreases with increasing velocity.
4. Frictional force depends upon the nature of the materials in contact.

Oregon Section

The fiftieth meeting of the Oregon Section of the American Association of Physics Teachers was held on February 12, 1949 at Oregon State College, Corvallis, Oregon. Dr. Walter P. Dyke, *Linfield College*, President of the Oregon Section, presided during the meeting. Dr. W. V. Norris, *University of Oregon*, reported upon the 18th Annual Meeting of the AAPT in New York, January 27–29, 1949.

Five contributed papers were presented at the meeting:

Difficulties in offering a physical science course. June Phillpot, *Salem High School.*

Where high school and college physics meet. Thomas A. Shotwell, *Vanport Extension Center.*

On simplyfying physics. Richard R. Dempster, *Oregon State College.*

Alpha- and gamma-ray spectroscopy. E. G. Ebbighausen, *University of Oregon.*

Atmospheric ozone. Fred W. Decker, *Oregon State College.*

Sigma Pi Sigma, physics honor society, held "open house" demonstrations of equipment and experiments in several shops and laboratories of Oregon State College. Luncheon was held in Memorial Union Tea Room.

Fred W. Decker, *Secretary*

Wisconsin Section

The annual meeting of the Wisconsin Section of the American Association of Physics Teachers occurred at Lawrence College, Appleton, Wisconsin on Friday and Saturday, May 6 and 7, 1949. All sessions were held in Science Hall. Professor R. R. Palmer, Wisconsin Chapter Representative, reported upon the meeting of the Executive Committee of the AAPT in New York. Three papers presented at the 18th annual meeting of the AAPT were reviewed:

How world war II has affected the science of physics. Lee A. DuBridge, *California Institute of Technology.* Review by T. A. Rouse, *University of Wisconsin at Milwaukee.*

Friction, a brief review. G. P. Brewington, *Lawrence Institute of Technology.* Review by Donald Olson, *Superior State Teachers College.*

Laboratory experiments on parabolic projectiles. Floyd W. Parker, *Lincoln Memorial University.* Review by Willard J. Pearce, *University Extension Division, Milwaukee, Wisconsin.*

At the session Friday evening Professor G. K. Willecke, *Lawrence College*, demonstrated a sound reproduction system developed by himself. Professor R. R. Palmer demonstrated a brilliant point source of light. A motion picture *Formation of Crystals in Polarized Light* prepared by Professor K. H. Bracewell, *Hamlin University*, was shown. Other films shown were *The Story of Palomar*, *Bottle of Magic*, and *Voice Sentinel*.

At the Saturday morning session contributed papers were presented:

Nature and objectives of the physics program at Lawrence. W. P. Gilbert, *Lawrence College.*

Some optical properties of paper. George R. Sears, *Institute of Paper Chemistry.*

Explanations of action in simple cell (given in physics textbooks). E. H. Schrieber, *Superior State Teachers College.*

A simple electronic timer. R. R. Palmer, *Beloit College.*

Review of recent proposals concerning nuclear structure. J. G. Winans, *University of Wisconsin.*

W. P. Clark, *Secretary*

What About Friction?

Frederic Palmer
The Franklin Institute Laboratories for Research and Development, Philadelphia, Pennsylvania

In Part I the reasons for accepting the validity of the "classical laws" of friction have been given, the sources quoted, and the laws stated as follows:

1. Frictional force is directly proportional to load, that is, to the total force which acts normal to the sliding surface.
2. Frictional force for a constant load is independent of the area of contact.
3. Frictional force is independent of the velocity of sliding.
4. Frictional force depends upon the nature of the materials in contact.

These laws are accepted by one school of modern thought.

In Part II reasons are given for believing that the intermeshing of surface irregularities is inadequate as a theory of friction. Several theories are proposed all of which depend upon molecular attraction as the basic cause. It is found that the surface irregularities in the area of actual (not apparent) contact are plastically deformed. Frictional force does appear to depend upon velocity. The phenomenon of stick-slip is discussed.

It is found necessary to restate the "classical laws" of friction as follows:

1. Frictional force is independent of load.
2. Frictional force is directly proportional to the area of actual contact.
3. Frictional force depends upon the velocity of sliding and in the higher range decreases with increasing velocity.
4. Frictional force depends upon the nature of the materials in contact.

PART III

Friction Saw. Electrical Theory. Lubrication

In Part I the reasons were discussed for accepting the validity of the "classical laws" of friction; and in Part II evidence was presented which led to the adoption of a molecular theory of friction. This theory was very useful in explaining frictional behavior connected with area of actual contact, decrease of friction at high speeds, and stick-slip. In the following sections further phenomena accompanying the decrease of friction at high speeds will be described, and a third theory of sliding friction will be discussed.

12. The Friction Saw

How can an iron disk (diameter from 2 to 5 feet), rotated at high speed, cut through a steel plate without even touching it? Perhaps it cannot, but a hundred and twenty-five years ago[1] it was so described by a reliable witness, the accuracy of whose observation has since been confirmed.[2] Either a band saw or a friction disk with a peripheral speed between 200 and 300 ft/sec will cut hot rails and bars, armor plate, carbon steel, aluminum, plate glass, Pyrex glass, plastics, carbide tool tips, wood that has been plastic impregnated, and many other materials, but it will not cut either stellite or rubber of any kind.[3] Figure 8 shows such a saw in action. Whether the saw touches the material to be cut or not is a question.

The heat generated by the friction saw is confined to a very small area, and when the velocity of the saw exceeds about 200 ft/sec, heat is produced faster than it can be absorbed through that area; hence the surface of the material is softened, melted, or burned locally, and the hot sawdust is removed by the sweep of the saw. Thus, the *work* does not become hot, because the heated portion is constantly and swiftly removed; and the saw is kept relatively cool, since each section cools during the 99.8 percent of the time it is not in the kerf.[4] The brilliant sparks seen in sawing steel are globules of iron oxide that form (see Fig. 9) and often explode due to the heat

[1] H. Daggett, "On the cutting of steel by soft iron," *Am. J. Sci. and Arts* **6**, 336 (1823).

[2] E. D. Sewall, "The toothless cold saw," *Iron Age* **76**, 1676 (1905).

[3] A. A. Schwartz, "Friction sawing," *Catalcgue from Tannewitz Works, Germany* (1945).

[4] A. A. Schwartz, "Metal sawing by frictional heat," *Am. Machinist* **87**, 94 (1943).

generated by the saw. Yet parts of the metal, the small shavings (seen in Fig. 10) are not in a molten state when removed.

Some friction saws are toothless, while others have blunt teeth set so as to resist sidewise flexure. The chief function of the teeth in cutting steel is to carry enough oxygen to the kerf to maintain the cobalt blue-white flame of burning steel, which enables the saw to cut efficiently without being damaged. The existence of such a flame may have misled those observers who think that an actual separation of the surfaces is essential to the action of the friction saw. Yet in a recent scientific journal there is the following statement: "Actual contact between the saw and the work would wear such blades out in no time; therefore the best guesses are that practically no contact takes place."[5] Such guesses appear to be not unreasonable if one realizes how high is the peripheral speed of a friction saw and assumes that the fluid (air and metal vapor) between the saw and the metal being cut may, under such extreme conditions, have enough shear to cause the removal of metal.

13. *Friction in Gun Barrels*

The problem of the effect of friction upon the motion of a bullet in a gun barrel has, for a long time, puzzled those who are especially interested in gun design and interior ballistics. As the speed of the bullet increases with nonuniform acceleration from zero to a muzzle velocity assumed to be about 3000 ft/sec, the coefficient of friction falls (Sec. 9); but how and why? What is its value at the muzzle velocity? Does it approach zero as the velocity approaches infinity?

These are questions that nobody can yet answer with certainty. They have been attacked experimentally in France and Germany,[6] but the results obtained are still open to question. Several authors have suggested an equation showing a relation between μ and v similar to that given in Sec. 9, but there is not agreement as to the values of the constants involved. An exceedingly interesting attempt has been made to measure the coefficient of friction of steel on steel at various muzzle velocities up to about 3000 ft/sec from which the value at infinite velocity can be calculated.

The method adopted[7] was to compare the muzzle velocity of a solid bullet fired in a smooth bore with the muzzle velocity of a bullet of the same weight that had its rear half nearly all bored out. The thin cylindrical wall of steel that remained was capable of being expanded by the powder gases so as to exert an increased force against the bore wall. The result was an increase in frictional resistance and a reduction in muzzle velocity. If it is assumed that the increased frictional force is the sole cause of the lower muzzle velocity, the relationship between the coefficient of friction and the muzzle velocity can be expressed by the following equation:

$$\mu = \mu_0 \frac{1+av}{1+bv} = 0.27 \frac{1+0.0044v}{1+0.064v},$$

in which μ_0 is the coefficient of friction for a very low velocity, v is the muzzle velocity, and the values of the constants are those for steel on steel.

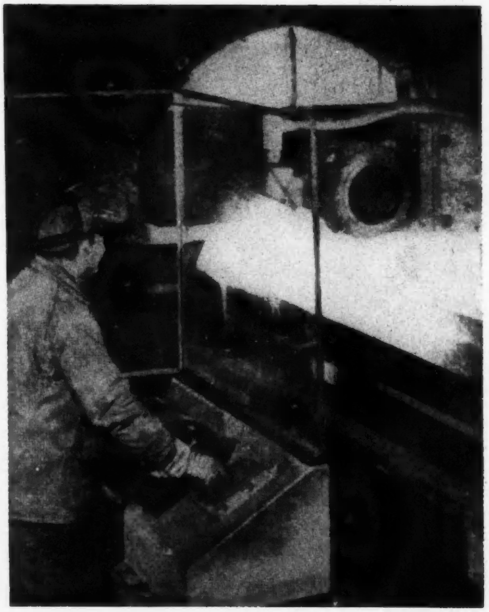

FIG. 8. Friction saw in action (*Mech. Eng.* **71**, 161 (1949)).

[5] "Friction sawing," *Scientific American* **176**, 111 (1947).

[6] The situation at the beginning of 1939 has been summarized by K. H. Bodlien, *Zeits., f.d. Gesamte Schiess und Sprengstoffwesen*, Vol. **34**, No. 2 (Feb. 1939).

[7] G. Grötsch and E. Plake, "Determination of the coefficient of friction of steel on steel at high velocities," *Z. G. S. S.* **35**, 3 (1940) and **35**, 30 (1940).

When v is approximately 3300 ft/sec, μ is 0.021; and for infinite velocity

$$\mu=\frac{a}{b}\mu_0=\frac{0.0044}{0.064}(0.27)=0.0185.$$

The curve representing the equation is shown in Fig. 11, as derived from four experimental values. The conclusions reached by the authors, as translated from the German, are as follows:

> The result of this work has been to establish that the coefficient of dry sliding friction of steel on steel amounts, at velocities of several hundred meters per second, to less than a tenth of its initial value at the start of the motion. This result is not necessarily limited to the case here considered, but would be generally applicable to the coefficient of friction between any two solid bodies sliding on each other. The following explanation may be suggested: for slow relative motion, the bodies sliding over each other under opposite normal forces enter into more intimate contact than is the case when there is only very brief contact of any two surface elements. The surface irregularities can then interlock more thoroughly, and also there is time enough for the topmost layers to become flattened out at the points of contact; hence a high velocity would act in a manner similar to a lubricating medium. This also explains why as velocity increases the frictional coefficient drops rapidly at a comparatively low velocity, without however, subsequently decreasing much more at high velocities. It assumes instead a constant value practically independent of the velocity. In the case of sliding friction with lubrication, it would similarly happen that there would be no dependence of the frictional coefficient on the velocity, at least in so far as the external frictional force is replaced by the internal friction of the lubricating medium.

This is very illuminating and of especial interest when a comparison is made between the behavior of the friction saw and that of a banded bullet in a rifled bore as follows:

1. While cutting, the saw remains cool since it is in the kerf for only a small fraction of a second; while being fired, the gun remains cool (so far as absorption of frictional heat is concerned) since the bullet is in the bore for only a small fraction of a second, though the quantity of heat going into each may be nearly the same.
2. Heat is largely concentrated at one spot on the "work"—the kerf; heat is largely concentrated at one spot on the bullet—the rotating band.

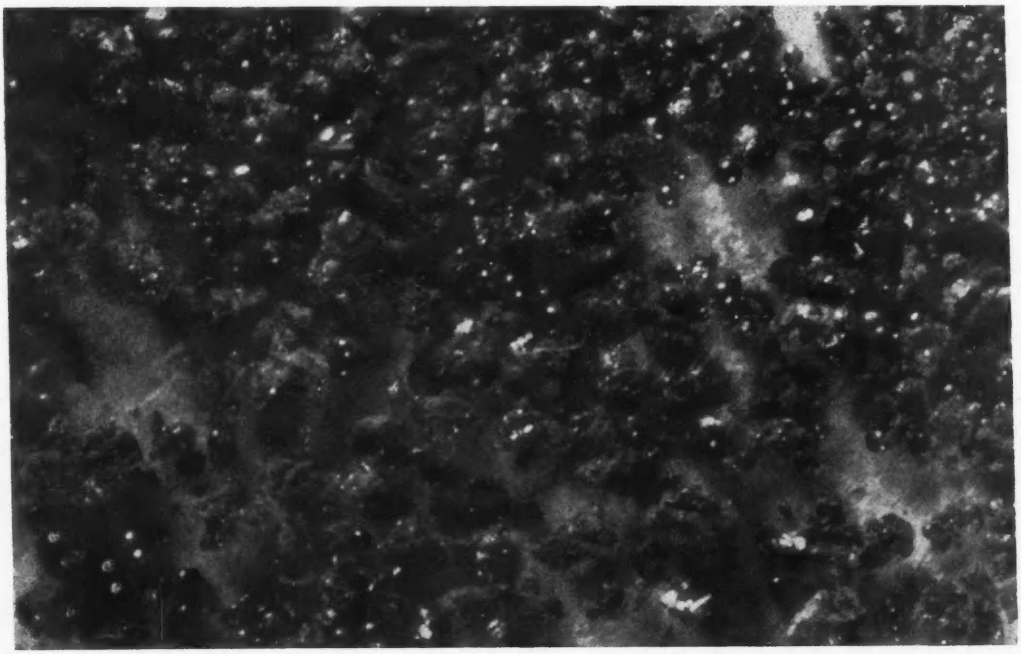

FIG. 9. Steel sawdust from friction saw showing spherical globules 25× (by Franklin Institute).

FIG. 10. Steel sawdust from friction saw showing shavings 10× (by Franklin Institute).

3. Material from the "work" is made plastic (Fig. 10), partly melted (Fig. 9), and partly vaporized, as seen from the color of the flame; copper from the rotating band is made plastic partly melted and partly volatilized as seen in the muzzle flash. It must be kept in mind, however, that much of the heat required to bring this about in the gun comes from the powder gases and not from friction.
4. When the "work" contains copper, the saw becomes coppered; when the rotating bands contain copper, the gun bore becomes coppered.

These similarities make it appear probable that the authors cited above[7] are right in concluding that at high speeds (above 250 ft/sec, or 80m/sec) sliding takes place as if the surfaces were separated by a lubricating medium (melted or vaporized metal). A similar result obtained at lower speeds, before actual melting takes place, has been described by another team of observers[8] as follows:

> It may seem surprising that the heat input of the bore should be so much less at high speeds (60 ft/sec) than at low speeds (40 ft/sec), since one would expect the frictional energy loss to be substantially independent of velocity. However, one should consider that with an increase in the velocity of the projectile, the temperature at the interface also rises and, as the softening point of the material of the bullet jacket (gilding metal) is approached, the value of the coefficient of friction between the latter and the bore of the rifle barrel declines, with the result that the energy dissipated in friction is reduced in the same proportion.

Much more work is needed to verify a relation such as that shown in Fig. 11, and it is important that such work should be extended to include the high-velocity sliding of various materials, one on the other, by more direct methods.

14. Electrical Theory of Friction

For more than twenty-five centuries it has been known that amber rubbed with cloth acquires the ability to attract light objects. The amber is found to be negatively and the cloth positively charged. More modern instances of this phenomenon (triboelectrification) are found in the paper industry, where the paper is heavily charged as it leaves the rolls; and in the oil industry, where each tank-truck drags a bit of chain on the ground in order to prevent a dangerous charge from accumulating as the result of the rubbing of the tires on the roadway. It is now generally recognized that any two different materials brought into contact and then separated

[8] E. L. Armi, J. L. Johnson, R. C. Machler, and N. E. Polster, "Application of the sliding thermocouple method to the determination of temperature at the interface of a moving bullet and a gun barrel," *J. App. Physics* **18**, 88–94 (1947).

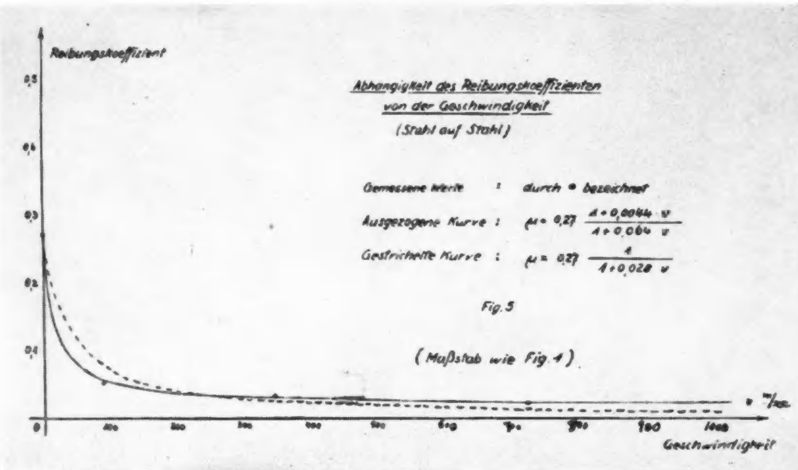

FIG. 11. Dependence of coefficient of friction upon velocity (steel on steel) (by Grötsch and Plake, reference 7).

are charged oppositely, the negative material having stolen electrons from the positive. Similarly, if a portion of a single material is suddenly torn to pieces, some of the pieces are negatively and some positively charged; as the droplets from an atomizer, the spray from a waterfall, or the dust in a coal mine.

Since one dry surface in sliding over another forms intimate contacts which are rapidly broken and reformed (Sec. 7), it may well be that triboelectric forces play a part in causing frictional phenomena. This possibility has been considered by surprisingly few investigators. One observer, however, who supports the electrical theory, states this thesis in very definite terms: "Triboelectricity and friction are two aspects of the same event: the rupture of the combination between atoms of solid surfaces"[9] and again: ". . . the parting of solid surfaces invariably gives rise to electric separation even when the two materials used are identical insulators or identical metals."

The magnitude of the charge separated during rubbing depends upon the nature of the surrounding gas. As gaseous pressure and humidity are decreased the charge is increased, but in an atmosphere of oxygen it is greatly decreased.[10, 11] The well-known strong affinity of oxygen for electrons may account for such a decrease, since negative oxygen ions would be captured by the positive metallic surface and neutralize its charge. Whenever chemical combination is possible from such a union, a layer of oxide is formed (Sec. 15).

Stick-slip can be explained in terms of the electrical theory since cycles of slow surface charging and rapid discharging may occur and cause relative motion to proceed by cycles of slow sliding and rapid slipping. The size of the jumps may be reduced by half if a small current of electricity is passed through the sliding surfaces.[12]

Whereas those who advocate the electrical theory are inclined to attribute frictional resistance entirely to the forces brought into play by the separation of opposite charges, it seems much more likely that such a mechanism is only one of several that enter into the over-all production of sliding friction.

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[9] P. E. Shaw, "The nature of friction," *Phil. Mag.* (7), 9, 628 (1930).

[10] J. A. Jones, "The influence of the surrounding medium on frictional electricity," *Phil. Mag.* 50, 1160 (1925).

[11] R. Schnurmann, "Sir Ambrose Fleming's method of electrification and Alfred Coehn's electrostatic experiments," *Proc. Physical Soc., London* 52, 179 (1940).

[12] R. Schnurmann and E. Warlow-Davies, "The electrostatic component of the force of sliding friction," *Proc. Physical Soc.* 54, 14 (1942).

15. Films of Oxide and Adsorbed Gases

Whether or not a film of oxide forms on a metallic surface in the manner described in the last section is of less importance than the effect of such a film upon frictional resistance. If the oxide coating adheres tightly to the underlying metal, friction is markedly lower than when the metal is uncoated.[13] The molecular forces are weakened probably because they are partially neutralized by the oxide coating.

Furthermore, oxide-coated balls of cast iron or mild steel show a much slower rate of wear than similar balls of hard steel. Apparently the oxide clings to cast iron or mild steel forming a protective layer. Subsequent rubbing then proceeds over the oxide layer which shields the ferrous surface from direct contact with oxygen. On the other hand the oxide cannot long cling to the surface of hard steel. It flakes off in the form of debris leaving the steel bare and subject to attack by more oxygen, hence to the removal of more metal.[14]

It is probably the accumulation of oxide that makes the "wearing-in" of ferrous surfaces effective. The formation and the retention of oxide tends to prevent welding and thereby to lower friction. This hypothesis has received confirmation through the examination of steel surfaces by x-rays[15] and of cast iron surfaces by electron beams.[16] If the oxide coating is thin, it will be continually broken through during sliding, hence the frictional force will be higher than when the coating is thicker.

Other films (sulphides, phosphides, etc.) have a lubricating effect similar to that of the oxides.[17]

Most measurements of friction coefficients have been made upon surfaces in air at atmospheric pressure with no consideration given to the possible lubricating effect of an adsorbed gas layer; hence it is not surprising to find that metallic surfaces cleaned and heated *in vacuo* (though not even thoroughly outgassed) exhibit very large coefficients of friction ($\mu = 5$ or more). Admission of either hydrogen or nitrogen to the vacuum chamber does not change the coefficient of friction; but admission of the least amount of oxygen has the same effect as an oxygen atmosphere, namely, a marked lowering of the coefficient of friction.[18] Contamination of the metal by mercury vapor has the same effect.

Those who contemplate conducting their own friction tests should bear in mind that consistent results are difficult to obtain, since most available surfaces are contaminated by oxide films or adsorbed gas layers. Furthermore, exposure of specimens to the vapor of either mercury or sulphuric acid will contaminate them still more, reducing the coefficient of friction by many percent. On the other hand, carbon tetrachloride, and smoke from cigarettes or burning wire insulation, even when highly dispersed, may double or triple it.

16. Lubrication

The principle of lubrication is not new. It has been known for two thousand years that a thin layer of fat or oil on the surface of a solid will make it slide easily. Since the time of Coulomb (Sec. 2c) many observers have tried to explain the mechanism of lubrication in terms of his theory of asperities. A modern modification of this theory has been recently expressed essentially as follows:[19]

1. The lubricant fills up the hollows in the sliding surfaces; thus the micro-deformation and shearing off of asperities is reduced, hence frictional force is less.
2. The number of points of actual contact is increased; thus the load on each one is decreased and its deformation is less, hence frictional force is less.

However, if the point of view is adopted that friction is due to the interlocking of molecular or electrical forces, the effect of the lubricant is probably to diminish the attractive forces of the

[13] W. E. Campbell, *Remarks; proc. conf. on friction and surface finish* (M. I. T., Cambridge, 1940), p. 197.
[14] W. A. Webb, "The influence of iron oxide on wear of rubbing surfaces," *Science* 99, 369 (1944).
[15] S. J. Rosenberg and L. Jordan, "The influence of oxide films on the wear of steels," *Trans. Am. Soc. Met.* **23**, 577 (1935).
[16] G. I. Finch, "The structure of sliding surfaces," *Engin.* **159**, 215 (1944).
[17] T. P. Hughes and G. Whittingham, "Influence of surface films on the dry and lubricated sliding of metals," *Trans. Faraday Soc.* **38**, 9 (1942).
[18] F. P. Bowden and T. P. Hughes, "The friction of clean metals and the influence of adsorbed gases. The temperature coefficient of friction," *Proc. Roy. Soc.* **172A**, 263 (1939).
[19] A. Mitinsky, "Fundamental considerations regarding friction," *Metal Progress* **53**, 102 (1948).

sliding surfaces and thus to lessen their capacity for cohesion, thereby lowering the frictional force. The magnitude of the effect brought about by the presence of an invisible layer of oil makes it very unlikely that the theory of asperities can account for more than a small fraction of the total frictional resistance.[20]

Confusion has sometimes arisen because the observer failed to recognize the significance of the thickness of the lubricating film. This thickness determines the characteristics of two types of oil lubrication: (1) fluid (thick film or hydrodynamic) lubrication, where friction takes place entirely within the lubricant, and is, therefore, dependent upon the viscosity of the oil; and (2) boundary (thin film) lubrication, where the film is so thin that the influence of the fields of force in the opposing surfaces is still significant.

The frictional behavior of metal surfaces with boundary lubrication is largely governed by the extent to which the lubricating film breaks down and allows metal to be torn to a depth that is large compared with molecular dimensions. This permits surface wear to take place and frictional force to be influenced greatly by the bulk properties of the metal boundaries.[21] It is difficult to establish conditions where frictional force and wear are simultaneously low.

Much has been written about the properties of different lubricants and the conditions under which they should be used; however, no attempt will be made here to do more than indicate a few of the materials that have been found useful: (1) fats (lard, suet); (2) mineral oils; (3) fatty acids with polar molecules (stearic, oleic); (4) extreme pressure oils with "additives"; (5) metallic films, vaporized (barium); (6) metallic films, extruded (lead alloys); and (7) graphite (molded or collodial).

Graphite is an excellent lubricant for steel since the coefficient of friction is low (0.065), it has good extreme pressure properties, and functions over a temperature range of 200°C.[17] A recent study of its peculiar properties offers an explanation of its behavior as a lubricant, and throws new light on the mechanism of friction.[22]

17. Conclusions

What, then, about friction? The answer is that in spite of both classical studies and recent advances there is hardly a single phase of the subject which is not still in the controversial stage. Authorities are in general agreement as to the nature of heat, the dual character of light, x-rays, radioactivity, and relativity,—all topics of comparatively recent origin; but there is no such agreement with regard to friction. A recent reviewer[23] has concluded his remarks thus:

> "Perhaps the above examples will indicate the various methods of approach and the differing points of view. It is again true to say that the apparently simple everyday phenomena are often those which turn out to be the most complicated and difficult to interpret."

In the present discussion it has been shown how frictional forces have, at one time or another, been ascribed to:

1. the intermeshing of gross surface irregularities, or asperities;
2. the action of molecular forces that cause adhesion, cohesion, or local welding;
3. the plowing of harder projections through softer ones;
4. the action of electrostatic forces.

Current opinion of the best qualified authorities seems to incline toward the view that all four of these mechanisms may participate to some extent in the production of frictional phenomena, but that the principal role is played by molecular forces.

The student should not be discouraged because the classical laws upon which he first pinned his faith have been proved either erroneous or largely inadequate. This is a symptom of advancing knowledge. He should be thrilled by finding that the subject of friction, though old in years, is still young so far as an understanding of its fundamental phenomena is concerned. By carrying on research in this field of such great practical importance there lies an opportunity for him to make a significant contribution to the advancement of scientific knowledge.

[20] W. B. Hardy, "The theory of lubrication," *4th Rep. on Colloid Chem. Dept. of Sci. & Ind. Res.* (Brit. Assoc. for Adv. of Sci. 1922), p. 185.

[21] F. P. Bowden and L. Leben, "The friction of lubricated metals," *Phil. Trans. Roy. Soc.* **239A**, 1 (1946).

[22] R. H. Savage, "Graphite lubrication," *J. App. Physics* **19**, 1 (1948).

[23] F. A. Vick, "Friction between solids," *Sci. Prog.* **35**, 484 (1947).

Dirac's Theory of Magnetic Poles

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ACCORDING to accepted electromagnetic theory,[1] magnetic poles—that is, entities on which magnetic lines of force begin or end—do not exist. Their nonexistence is stated in one of the field equations, *viz.*,

$$\mathrm{div}\mathbf{H}=0. \tag{1}$$

Recently Dirac[2] has built up a new electromagnetic theory on the hypothesis that there can be magnetic poles. This theory is quite remarkable in that it leads to the conclusion that electric charge is quantized, that is, that every charge that can exist must be an integral multiple of a fundamental charge of fixed value. The quantization of charge is a well substantiated empirical fact of long standing, but it had not hitherto been accounted for theoretically.

Dirac's paper employs rather unusual mathematical methods and is not easy to read; but the basic physical ideas are relatively simple, so that it is possible to reformulate the main points of the theory in fairly elementary terms. The object of this paper is to carry out such a reformulation.

Fundamental Hypotheses of Dirac's Theory

1. It is assumed that magnetic poles can exist, a pole of strength g having $4\pi g$ lines of force beginning or ending on it. Mathematically, this means that the right side of Eq. (1) can no longer be zero but must be set equal to 4π times the pole strength density.

2. It is assumed that a moving pole produces an electric field, just as, in ordinary electromagnetic theory, a moving charge produces a magnetic field. This assumption is necessary in order to preserve the symmetry between electric and magnetic phenomena to the extent required by relativity theory. Mathematically, it means that another of the Maxwell-Lorentz field equations,

$$\mathrm{curl}\mathbf{E}=-(1/c)(\partial\mathbf{H}/\partial t), \tag{2}$$

must be altered, for a term proportional to the magnetic current density must be added to its right side.

3. The force exerted by an electromagnetic field on an electric charge is assumed to be given by the same expression as in the ordinary theory; and the force on a magnetic pole is assumed to be given by a similar expression in which the roles of electric and magnetic quantities have been interchanged.

Development of the Theory

In order to develop ordinary electromagnetic theory into a quantum electrodynamics, the first step is to express the field equations in terms of a vector potential and a scalar potential. The possibility of defining these potentials rests on the fact that Eqs. (1) and (2) are fulfilled; for example, the vector potential is defined as a vector field whose curl is equal to $\mathbf{H}$, and there cannot be any such field unless Eq. (1) holds at all points.

In the new theory, potentials can be defined for fields produced by electric charges, since Eqs. (1) and (2) are fulfilled by those fields. A field due to magnetic poles, however, satisfies the modified equations rather than Eqs. (1) and (2), and so it cannot be expressed in terms of the potentials. Nevertheless, such a field can always be resolved into two partial fields, one of which can be expressed by the potentials; the other part would then have to be described by some other device. The problem is, how to carry out the resolution in the most economical way. In the case of poles at rest, which is the only case we shall consider, the answer is given by the theory of permanent magnets. It will be recalled that the field $\mathbf{H}$ of a permanent magnet can be considered the resultant of two parts. One part is the magnetic induction $\mathbf{B}$, which is solenoidal and can therefore be expressed as the curl of a vector potential; while the other part is $-4\pi\mathbf{I}$, where $\mathbf{I}$, the intensity of magnetization, differs from zero only inside the magnet. In Dirac's theory, the magnetic field of a pole at rest is resolved in exactly the same way into a solenoidal part and a part that differs from zero only in a limited region of

[1] We have in mind here the Maxwell-Lorentz theory, rather than the phenomenological theory. It will be recalled that the Maxwell-Lorentz theory uses only one electric field and one magnetic field, $\mathbf{E}$ and $\mathbf{H}$, respectively, whereas the phenomenological theory uses two electric fields, $\mathbf{E}$ and $\mathbf{D}$, and two magnetic fields, $\mathbf{H}$ and $\mathbf{B}$.

[2] *Physical Rev.* **74**, 817 (1948).

space, which, for simplicity, is taken to be a geometrical line or curve ("string"), extending from the pole to another pole of the opposite kind. A string may have any arbitrary shape and we may think of it, if we wish, as a slender tube rather than a geometrical line.

Thus all parts of the electromagnetic field due to charges and poles can be described by the potentials, except the part corresponding to intensity of magnetization. How is this part to be described? By analogy with the permanent magnet, we shall have a complete description of it if we know the shape and position of each string and the strengths of its sources, which are the magnetic poles. The shape and position of a string can be specified by giving the position coordinates of each point on the string; and accordingly these are taken as the variables describing the nonsolenoidal part of the magnetic field. It is worth noting here that the word "coordinate" in modern physics is used in two somewhat different senses: in the dynamics of particles the coordinates are functions of the time which specify the *positions* of the particles, while in electromagnetic theory the coordinates of the field—which are ordinarily the potentials—are functions of time and position which specify the *state* of the field at every point, but do not give the position of any physical entity. We see, then, that in the new theory the variables chosen to describe the part of the field corresponding to intensity of magnetization are essentially coordinates in the first sense; and it follows that this part of the field is to be treated by the methods of *particle* dynamics.

The Quantization of Charge

Consider an electric charge e and two magnetic poles g and $-g$, all at rest. To simplify some geometrical details, we shall take them to be in line, as in Fig. 1, and we shall take the string connecting the poles to be all in one plane, although the argument can be carried through without imposing these conditions by modifying the procedure somewhat.

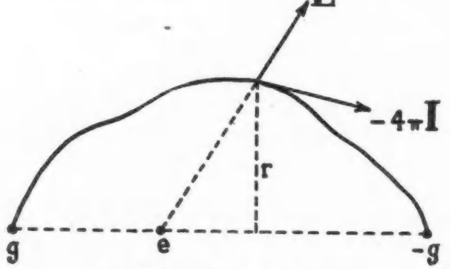

FIG. 1. Quantization of charge.

By analogy with a permanent magnet, we see that the flux of the nonsolenoidal part of the magnetic field (that is, of $-4\pi\mathbf{I}$) at any point on the string will be $4\pi g$, its direction being along the string. This flux and the electric field from the charge will give rise to an electromagnetic momentum per unit length of the string, of value

$$(1/4\pi c)(\mathbf{E}\times\text{flux of}-4\pi\mathbf{I});$$

and therefore there will be an angular momentum about the axis through the poles. Let us suppose the string to rotate about this axis without changing its shape, carrying both parts of its magnetic field along with it; this will keep the *resultant* magnetic field at every point constant, for the resultant field is determined only by the positions and strengths of the poles, as one can see by analogy with a permanent magnet. Let θ be the angular displacement of the string from some fixed reference plane. Since the nonsolenoidal part of the magnetic field is to be described by the methods of particle dynamics, we may require, in accordance with the Bohr quantum condition, that the phase integral $\int_0^{2\pi} p d\theta$, where p is the angular momentum of the nonsolenoidal part, be an integral multiple of h.

This phase integral may be evaluated as follows: The contribution to it from an element of string dl rotating through $d\theta$ will be $(1/4\pi c)(E_n)(4\pi g)rd\theta dl$, where E_n is the component of $\mathbf{E}$ normal to the surface element traced out by dl. This reduces to $(g/c)E_n dS$, where dS is the surface element. This is to be integrated over the entire area traced out by the string in one revolution, and the result, by Gauss' theorem, is $(g/c)4\pi e$. Thus the quantum condition leads to the result:[3]

$$4\pi eg/c = nh. \tag{3}$$

It may be objected that we have not taken into account the contribution to the phase integral of

[3] Our derivation of Eq. (3) is an adaptation of Dirac's method. Professor H. A. Wilson has given a more straightforward derivation, in which the angular momentum is first computed explicitly for a fixed position of the string and found to be $2eg/c$; on integrating this with respect to θ from 0 to 2π, one obtains $4\pi eg/c$.

the solenoidal part of the magnetic field. This is zero; and the reason is as follows: The coordinates of this part of the field, as already noted, are the potentials at the different points in space; and the generalized momenta conjugate to them are certain of the components of the entire field. As we have already seen, the entire field remains constant at each point during the rotation of the string, while the potentials vary, but return to their original values on completion of the string's cycle. Thus the phase integral from the solenoidal part of the field has a constant integrand, and the integration variables return to their original values; therefore the integral is zero. The reason why $\int_0^{2\pi} p d\theta$ was not zero was that θ did not return to its original value.

It has been pointed out by H. A. Wilson[4] that Eq. (3) can be arrived at without using strings at all.

The quantization of charge can be deduced from Eq. (3) as follows: Let us bring poles of different strengths $g_1, g_2, g_3, \cdots$ successively into the field of a fixed charge e; then because of Eq. (3), $g_1, g_2, g_3, \cdots$ must be proportional to positive integers, so that we conclude that pole strength must be quantized. Now let us take a pole with strength equal to the smallest of the allowed values just determined (i.e., $hc/4\pi e$), and bring different charges into its field. Because of Eq. (3), these charges must be proportional to positive integers. Thus charge is quantized.

If the smallest allowed value for a charge is identified with the charge on the electron (e_0), then the smallest pole strength (g_0) will be, according to (3), $hc/4\pi e_0$. If the numerical values of the physical constants are put in, this makes g_0 about 70 times e_0.

In this discussion we have studied only the case in which all poles and charges are at rest; Dirac's paper treats the general case. Furthermore, we have not discussed the details of the formulation of Dirac's quantum electrodynamics.

The author is indebted to Professor H. A. Wilson for a number of helpful suggestions.

[4] H. A. Wilson, *Physical Rev.* **75**, 309 (1949).

Formulation of Objectives of Teaching in the Physical Sciences*

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Thesis

THERE are two schools of thought on what constitutes the function of education. One of these relies on the native abilities and inclinations of the student. The fundamental assumption of this school seems to be that man is inherently good. The problem of education is consequently reduced to giving the student an *opportunity* to develop to the utmost his talents. Since students' talents and proclivities vary widely, a detailed and specific statement of the desired outcomes of education and therefore of the proper training must wait on the teacher's acquaintance with individual students.

The fundamental assumption of the other school of thought seems to be that either man is bad—or has bad in him that may become dominant unless controlled—or that our way of living is far removed from man's natural mode of life. The problem of education is therefore to *change* the young student into an adult that could function well in this particular society. The emphasis is here shifted from the analysis of what the student is like as he begins a course of training toward the postulation of what the student should be after completing his training. This approach makes possible rather detailed descriptions of goals and proper training that are the same for large groups of students.

The writer believes that the second formulation of the function of education holds more promise as a theoretical basis of higher education. He admits that little progress can be made without examining the raw materials of education, that is, the student himself. This examination may

* This is a part of a paper, *Testing for Specific Objectives of Physics Teaching*, presented at the January 1949 meeting of the AAPT.

legitimately influence the goals of education in general; in particular, it certainly should influence the goals set for the present generation of students who are to be prepared for living in this particular phase of civilization. The writer maintains, nevertheless, for theoretical and practical reasons that the first step toward the badly needed revision of American college education is for the colleges to state clearly the desired outcomes of higher education. It is the thesis of this paper that the objectives of a course of study or of a curriculum of a college should be stated explicitly and in considerable detail; that teaching should be directed consciously and specifically toward the achievement of these objectives, and that the degree of achievement of the objectives should be measured.

Objectives as the Guiding Principle

In specifying a curriculum or a course, three factors must be considered: the objectives to be achieved, the subject matter or content to be taught, and the method of instruction to be used. Although all educators recognize the existence of all three factors, the emphasis they lay on each varies a great deal. The extremists of the content-conscious school maintain that the choice of content, method, and objectives of a course in physics, to take an example, is uniquely and completely determined by the phrase, "one-year course in physics." The content of the course is here determined by the nature of the discipline—by what constitutes its basic, its essential parts. "The mastery of these parts" is considered a full statement of the objectives. Proper methods of instruction follow *a fortiori.*

The difficulties encountered in this approach are threefold. First, since the future of the student is not considered, the same course will be prescribed for a prospective physicist and a prospective poet. Unless the course is a part of a general education program, identical training for students with such divergent interests is difficult to defend. Second, the approach does not allow of a rational choice among the basic parts or of an intelligent treatment of the complementary relation between depth and breadth, for physics is surely both deep and broad. Few physicists would be willing to assert that one of the following is not basic to physics or less basic than the rest: concept of angular momentum, second law of thermodynamics, Joule-Thompson experiment, electromagnetic theory of light, to take examples out of classical physics only. If all such topics are "covered" in a one-semester or even one-year course of physics, the result is inevitably a *smorgasbord* of "basic" titbits that is painful to prepare and impossible to digest. The alternative is to omit some of the basic parts of physics, guided by tradition, convenience, or whim. The meal resulting from such omissions, to continue the metaphor, may be digestible, but not necessarily nutritious. Third, the statement of the objectives above is devoid of operational meaning; it offers guidance neither in teaching nor in testing.

The difficulties described above seem inherent in the content-conscious educational philosophy. These particular difficulties would disappear if properly stated objectives of the curriculum or course were taken as the guiding concept. The content would then be chosen on the basis of what *effect* its study would have on the student, not on the basis of how essential, basic, or important a part it is of the particular field.

The method-conscious educators maintain that there are inherently good or even inherently best methods of instruction. They argue further that, since the possible outcomes of training are determined by the nature of training, a statement of objectives is superfluous. Some of these educators believe that the best method of teaching science is to have students discuss original writings; others advocate a strong emphasis on laboratory work or on solving numerical problems; still others feel that the most important thing is to have the students come under the influence of instructors who are themselves creative scientists. All of these are admittedly highly reputable methods. But if the limitation of time necessitates a predominant choice of only one or two of these methods in a course or a part of the course, the method-conscious philosophy of education provides no logical path toward such a choice. A rational choice can be made, however, on the basis of the objectives. The prior question, as always, is: what is to be accomplished? Both the content and the method must, in the opinion of the writer, be justified in terms of the desired ends of education.

Desirability of an Explicit Statement of Objectives

All conscientious teachers have some objectives in mind but only few of them have gone as far as putting these objectives into words. An explicit formulation of the objectives of a given course is desirable and often necessary for three reasons: (1) to help the instructor in his choice of content and method of instruction; (2) to enable him to prepare adequate tests; and (3) to establish communication among the members of the staff teaching the course, between the staff and the administration, between the staff and students, and, unless each instructor prepares his own examination, between the staff and the examiner. A clear statement of objectives and a consideration of their relative importance will often lead the instructor to a better systematization of his ideas and to a critical re-examination of the content, method, and the time allowance for various topics and exercises in the course. In the matter of communication, it seems that the following questions cannot be fully answered without a specification of the objectives, of the content, and to some degree of the method of a course. Are all the members of the staff of a given course really teaching the same course? How good and proper a part of the curriculum is a given course? How should a student of a course apportion his time and effort? What are the requirements for passing the course?

Methods of Arriving at a Formulation of Objectives

There are two ways of arriving at a formulation of objectives of a particular course. One is first to state the most general objective. Here one must conquer the fear of uttering platitudes. The main objective may be to prepare the student for the next course or to help him become a good scientist or a good citizen. The next step may be to define those characteristics which the student must acquire or improve in order to qualify for the main objective. Here again one must put down everything of importance, even though certain qualities may not be directly teachable or are not taught, such as various attitudes and habits. The next step is to decide to which of these objectives a given field of study such as physics, or perhaps even more specifically a given course in physics, could contribute. At this level of specificity the objectives, so far stated in terms of the *qualities* a student should possess, are to be translated into the terms of *behavior* the student possessing such qualities should exhibit. Finally the subject matter to be taught and the method of instruction is chosen so as to insure the achievement of the objectives.

The above process—going from the general to the particular—is to be recommended to all groups charged with the construction of a curriculum, or with the design of a new course. Even in an established curriculum, however, the staff of a course should critically examine their course to see if it forms a coherent part of the plan of the department and of the college, and draw a tentative set of objectives that would insure such coherence.

The other method of arriving at a formulation of objectives is to go from the particular to the general. The first question to be answered here is what good behaviors may be expected of a student upon completing a good course in physics. Since the purpose of the course is not yet specified, the definition of "good" must be arrived at from a semi-intuitive recognition of possible objectives. The emphasis here lies on what is essential in physics *qua* physics, on what constitutes the core and the fabric of the field. Thus, for example, since physics is an exact science, the ability to solve numerical problems is a possible and perhaps desirable outcome of physics training. The next step is to identify those qualities in a student which are exhibited in the "good" behavior. Finally, those qualities are identified which are expected to contribute to the making of a good scientist, good citizen, or a good student in the next course.

A variation of the second method consists in choosing as a starting point the method of instruction rather than the content and structure of physics. Some teachers feel certain that a good training in science will result from reading and discussing original writings of physicists; others feel the same way about laboratory experience, etc. They find it easier and more profitable to analyze methods than content and structure, or to derive the specific objectives of a physics course from the general objectives of education.

These two sets of objectives must finally be compared, however.

The from-general-to-particular method seems the more logical but it presents certain formidable difficulties. The logical path from "creative scientist" or "enlightened citizen" to more specific objectives of a course in physics is not easy, unique, or unambiguous. The publication of the Harvard report, *General Education in a Free Society*, to give one example, shows that the difficulties are not insurmountable, and that agreement on objectives at varying levels of generality is possible even among a fairly large group of educators. But the process is hazardous and the danger is always present that the agreement may be on a verbal level only and not on the all-important level of actual instruction.

The danger of the sole use of the from-particular-to-general method lies in the temptation to avoid the final and painful step of matching the specific objectives against the general ones. Without such matching, the emphasis must remain on what is important in physics *qua* physics, and not on what characteristics are desirable in the future citizen or scientist. The two criteria will not in general lead to the same choice of content and method.

The key question of the from-general-to-particular method, as applied to objectives of a physics course, is what physics *should* contribute. The key question of the other method is what physics *could* contribute. It seems clear that to arrive at a defensible yet realistic set of objectives both methods must be used.

Form of the Objectives

The description of a course in terms of the objectives to be achieved may be quite lengthy, especially since it is usually desirable and often necessary to illustrate the expected behavior of the student by one or more examples, perhaps in the form of questions or exercises he should be able to deal with. It is, therefore, convenient to have in addition to the above lengthy description a list of briefly stated objectives. Great economy and clarity are achieved by making the list of objectives two-dimensional. The one dimension would consist of the description of the desirable behaviors of the student; the other would describe the subject matter or content within which the above behaviors would be expected to be exhibited. (The content dimension may be partly taken care of by a reference to a syllabus or a textbook used in the course.) Thus, for example, a behavior objective of a course may be to inculcate in the student "The ability to apply known principles in new situations." The content dimension should supply the names or statements of the principles in question. A distinction should be made between the content of the course and the content-dimension of the list of objectives. Thus, in a course in physics, both the kinetic theory of gases and caloric theory of heat may be expounded. Yet the knowledge of the former theory may be one of the objectives of the course, while the latter theory may have been introduced with the sole purpose of inculcating in the student the ability to judge the *cogency* of evidence; another theory may have done just as well.

In most courses with which the writer is familiar the content dimension is perfectly clear; what is not clear is what the student completing the course is expected to be able to *do* with the content, or to what extent he is expected to transcend it. This, it seems, cannot be made clear without specific statements about the behavior objectives of the course. Thus, in teaching about Newton's laws of motion some, if not all, of the following may be considered desirable behaviors: To know the statement of the laws (more precisely, to be able to state the laws verbally and algebraically); to be able to work numerical problems involving the laws (here, the generality of the problems and their similarity to those taught in the course must be specified); to know the relation of these laws to other laws of physics, such as conservation of momentum or energy; to know the historical development of the laws; to know the realm of applicability of the laws; to know the status of the laws (whether the laws are assertions of an empirical, theoretical, or definitional nature). These are examples of what the student may be expected to do with this particular content, namely Newton's laws. But it may also be desirable that the student transcend this content and be able to answer questions of a more general nature: How are definitions chosen in physics? What are the criteria of their excellence? What is their contact with reality; that is, to what extent may they be said to be true? Can a definition ever

be found to be wrong? To what extent does the choice of concepts and definitions color our knowledge of reality?

The behavior objectives enumerated in the preceding paragraph can be achieved—some of them only partially, of course,—through the study of Newton's laws. Some of the same objectives and additional objectives may be an expected outcome of the study of other laws and theories. An additional complication arises from the fact that not all contents are equally suitable for the attainment of a given set of behavior objectives. No teacher is expected to remember the content of a course; he is invariably provided with an outline or a syllabus. It seems equally unreasonable to expect a teacher to carry in his memory all the behavior objectives. A list of these is a requirement for any one but a genius. This list may have the same relation to a fuller description of the purposes of a course as an outline has to the textbook describing the content of the course.

A systematization of stated objectives is essential if these are to serve effectively their triple function of improving communication, teaching, and testing. Adequate classifications of content objectives are well known. One of such divides the field of physics into mechanics, heat, electricity and magnetism, sound and light; these sub-fields are further subdivided. Schemes of classification of behavior objectives are less well known and therefore merit discussion. The choice of the basis of classification is not unique. The writer is familiar with and has himself experimented with the following primary or fundamental bases: memory *vs.* mental skills; inductive *vs.* deductive thinking; quantitative *vs.* qualitative thinking; simple *vs.* complex mental processes; knowledge level, critical level, and creative level of abilities; and others. A choice of one of these bases as the primary one does not imply giving up the distinctions inherent in other bases. Thus, in the memory *vs.* mental skills scheme, mental skills may be subdivided into those required for inductive and deductive processes of thought; each of these may be applied to qualitatively or quantitatively described material; and so forth.

If the list of behavior objectives is to serve as an outline, it must avoid overspecificity; such, for example, as would result from including in the list "Ability to use Newton's laws of motion for rigid bodies sliding down an inclined plane without friction." At the other extreme, describing an objective as "Mastery of Newton's laws of motion," is of little practical use. A compromise between the extremes of specificity and generality must be made based on the proposed use of the list.

Possible Behavior Objectives

Below is given an example of a list of behavior objectives.[1] After the statement of some of the objectives, brief parenthetical comments and descriptions of possible test exercises are included. The intent of such inclusions is to make clearer the operational meaning of the objectives. Further clarification can be obtained by examining the test exercises included in the manual.

1. KNOWLEDGE

This objective has been variously described as: possession of information; grasp of the subject matter; acquaintance with the main facts; knowledge of the course; textbook knowledge; remembering what was taught in the course. The last is very nearly the sense in which the word knowledge is here used to designate this objective. The main ability to be tested for in the exercises under this heading, or at least the ability that can most reliably be tested, is memory. Success in the test will probably depend, however, on other abilities, notably on the ability to organize and relate the materials studied; this ability in turn depends on the student's intelligence. It must be emphasized, however, that the ability to organize is not to be tested for directly; the ability to organize is to be of help to the student primarily as a mnemonic device. The three main subheads 1.1, 1.2, and 1.3 are arranged in the order of increasing demand in this ability.

1.1 *Subject matter knowledge* (*straight memory questions*).

1.11 Knowledge of laws and principles (verbal and mathematical).

1.12 Knowledge of theories.

1.13 Knowledge of facts (e.g., density of iron).

1.14 Knowledge of technical terms, symbols, units, dimensions, etc.

1.2 *Analytical knowledge.* This is the knowledge of the relations or patterns studied in the course; it is thus of a more functional nature than subject matter knowledge (1.1). The ability to organize the materials of the course

[1] This list was included in, and formed the basis of, the manual *Testing for Specified Objectives of Physics Teaching* prepared by the present writer and distributed to the members of the AAPT before whom the present paper was read. The manual consists of a large number of test exercises designed to test for the behavior objectives included in the list. The writer will be glad to furnish copies on request.

should be of help to most students in retaining these relations. It is important to notice, however, that these relations or patterns are to be tested for in nearly the same context in which they appeared in the course; if the context or the situation were genuinely novel, not the memory of the relation, but the ability to discern it, would be tested. Such an ability in the writer's scheme of classification does not belong under the heading of knowledge.

1.21 Knowledge of the relation between empirical generalizations (laws of nature) and specific phenomena. (The generalizations are those taught in the course.)

1.22 Knowledge of the bases of theories; of relation between theories and facts. (The theories, assumptions, evidence, etc. are those taught in the course.)

1.23 Knowledge of experimental procedures: instruments; factors affecting the validity of the experiment; etc. (All of these having been taught in the course.)

1.24 Knowledge of the appropriate sources of information (whether to consult a textbook, a journal, etc.).

1.3 *Knowledge of methodology.* Here the student is to be tested for the knowledge of the structures of the separate physical sciences, the relation of these sciences to one another, and their relation to other fields. As in Sec. 1.2, only those structures and relations which were specifically taught in the course belong in this category.

1.31 Knowledge of the nature and structure of the physical sciences (or physics).

1.32 Knowledge of the historical development of the science.

1.33 Knowledge of the realm of the physical science (or physics) and of its branches.

2. ABILITY TO USE THE METHODS OF SCIENCE

Other phrases used to describe this ability are: working knowledge of science; critical thinking in science; mastery of the scientific method; possession of the tools of science; ability to act as a scientist; possession of mental or intellectual skills used by scientists. While in Secs. 1.1 to 1.3 the student is to be asked essentially to reproduce what he heard or read in the course, in this section it is desired to know what the student can do when more or less on his own. It is therefore necessary that the situations used contain elements that are new to the student. At the same time it is necessary, in general, that the situations be of the same kind as those studied in the course; otherwise, it is unlikely that the ability of the student to handle the situation could have been improved by the course. Thus it does not seem fair to ask a student whether a given experiment could be used as evidence for a given theory unless the course had presented some experimental evidence for some theory so that the student knows the general pattern expected. At the other extreme, if that particular experiment was quoted in the course as evidence for that particular theory, the item properly belongs in Sec. 1.2.

2.1 *Ability to use methods of science in abstract situations.* The exercises are to test the student's ability to use methods of science in situations that are well defined and clear cut and in which a minimum of content knowledge in the sense of Sec. 1.1 is required.

2.11 Ability to apply stated principles. (A principle is stated; the student is to apply it to a situation described.)

2.12 Ability to carry out symbollically indicated operations. Example: If $\textcircled{a} = a^2$, and $\boxed{a} = 1/a$, then $\frac{1}{\textcircled{\boxed{\frac{2}{3}}}}$ is equal to: A– $\frac{2}{3}$, B– $\frac{3}{2}$, C– $\frac{4}{9}$, D– $\frac{9}{4}$, E– none of these answers is right.

2.13 Ability to use syllogisms.

2.2 *Ability to use methods of science in "academic" situations.* These are situations which are new to the student; their complexity, however, is about the same as that of the situations used in the course for illustrative purposes. Usually a single principle, law, or theory is sufficient for the analysis of the situation.

2.21 Ability to relate empirical generalizations (laws of nature) and specific phenomena. (Although the student may be familiar both with the generalization and the specific phenomenon, the particular relation between these that is used in the test question must be one that was not taught in the course.)

2.22 Ability to relate theories and facts. (The relation between the theory and the facts of the test question must be new to the student.)

2.23 Ability to analyze and criticize an experiment. (The experiment described must be new to the student.)

2.3 *Ability to use methods of science in "whole" situations.* These are situations approximating those the student may face in real life—outside the classroom. They are characterized by greater complexity than the "academic" situations of Sec. 2.2. Exercises included in this section are to be limited to those situations which have not been taught in the course and which require more than one principle—or even more than one branch of science—for their analysis.

2.31 Ability to use methods of physics.

2.32 Ability to use methods of the physical sciences.

2.33 Science and Society. (Situations made complex by the mores, etc. of the community.)

3. ABILITY TO READ SCIENTIFIC LITERATURE

Here the ability to read is used in its widest possible sense; it ranges from understanding the literal meaning of particular sentences or the information conveyed by graphs, drawings, and tables to inferring the conceptual framework used by the author.

3.1 *Ability to read a book or a long article.* The book may have been read and discussed in the course or assigned for reading before the student came to the examination. The particular questions that appear in the test, however, should not have been discussed in class.

3.2 *Ability to read a passage.* The passage to be read and the questions asked should be so chosen that the student would not be able to answer the questions without a thorough understanding of the passage. Ordinarily the passage must be on a topic not taught in the course.

3.3 *Ability to interpret tables, graphs, drawings, etc.* Tables, graphs, and questions should be so chosen that the

student would not be able to arrive at correct answers merely from his previous knowledge of the physical principles involved.

3.31 Ability to interpret a table of values.
3.32 Ability to interpret graphical data.

4. PROPER ATTITUDES AND HABITS

It is possible to learn something about the student's attitude and habits of thought by analyzing the kind of *wrong* answers that he seems to prefer. The attitudes and habits listed below form no more than a sample.

4.1 *Attitude of overcautiousness vs. that of jumping to conclusions or going beyond data.*

4.2 *Attitude of underestimating the power and value of science vs. that of overestimating these or depending on the methods of empirical science in the fields of philosophy, religion, etc.*

4.3 *Attitude of underestimating the value of experiment or observation as tools of science vs. that of underestimating the importance of reason or of the man-made nature of science.*

4.4 *Possession of strong prejudices or preconceptions.*

4.5 *The habit of learning things well, or not at all vs. that of learning something of everything.*

The above list includes behavior objectives of several courses taught at the University of Chicago, ranging from some ten weeks of physics in a physical sciences course in the College to a one-year laboratory course given by the Department of Physics. The content dimension of the objectives varied widely from course to course and is therefore not included. The behavior objectives, on the other hand, were sufficiently similar to justify including them in a single list. It is hoped therefore that the above list of behavior objectives will be of value—primarily suggestive value—in a variety of physics courses.

Criteria Used in Making Up the List of Behavior Objectives

The classificatory scheme of behavior objectives used by the writer for his manual, and the form of the objectives themselves were chosen on the basis of the following criteria: communicability, importance, teachability, testability, comprehensiveness.

1. Communicability. Communication about objectives should be possible at the level of their implementation in teaching and testing. The experience of the writer with teachers in various courses in the physical sciences and in physics both at the University of Chicago and at several other institutions makes him believe that this criterion at the level of testing is fairly well met. Many of these teachers after one or two conferences with the writer, and after familiarizing themselves with the illustrative test exercises of the manual, were capable of writing their own test-exercises. These exercises, though technically imperfect, showed a good understanding of the meaning of the objectives thus tested. At the much more important level of teaching, the results are unfortunately not unambiguous. The writer attended many and varied lecture, discussion, and laboratory classes and has studied the results of many tests of students. His reluctant conclusion is that few instructors teach well for any behavior objectives other than objectives 1.1 and 1.2 of the list above. Yet, in discussing the matter with these instructors, it seemed to the writer that they did have a fair understanding of other objectives even to the extent of suggesting teaching situations appropriate for inculcating them. Thus adequate communication seems to have been established. The seeming failure of teaching for those behavior objectives which teachers themselves consider important may be due to the following factors. Most teachers of physics belong to the content-conscious school of education. A typical comment is: "I *had* to teach Boyle's law, Charles' law, etc. I forgot (or I had no time) to teach how to interpret data." The implication is clear that the *knowledge* of many laws (objective 1.11) is felt by these teachers to be more important than the knowledge of only one or two of the laws even if it is supplemented by a generalized ability to use such laws (objectives 2.21 and 2.23). There also are many teachers who subscribe to the "osmosis theory" of instruction. These teachers believe that students acquire, say, the ability to interpret data by watching the instructor interpret data at the blackboard. Fortunately, even these teachers do not extend the "osmosis" or "good example" method to the ability to solve numerical problems. They insist that the student try to do some problems on his own. One of the reasons for the difference in treatment of the two abilities is probably that numerical problems are much easier to write than exercises involving interpretation of data.

2. Importance. The objectives should be important ones. The writer has discussed objectives of physics and of physical science with literally

hundreds of persons including internationally famous physicists and chemists, college presidents, deans, heads of departments, and professors of physical sciences, humanities, social sciences, and biological sciences. At the level of *acceptance* of the writer's list of objectives as being important for both the future physicists and nonphysicists, the agreement was practically unanimous. Even at the level of comparison the agreement was good. Those instructors, who could be persuaded to learn the language of behavior objectives and use it in describing what they considered important outcomes of their teaching, wrote out lists of objectives that covered very nearly the same ground as does the writer's list. There was a good deal of difference of opinion, of course, as to the relative importance of individual objectives; teachers of preprofessional courses often placing greatest emphasis on knowledge (objective 1), while teachers of general courses usually favoring intellectual skills and abilities (objectives 2 and 3). The predominant basis of classification was that used by the present writer: knowledge *vs.* intellectual skills.

3. Teachability. The objectives must be realizable. It is difficult to assess the consensus of opinion as to whether it is possible in a course in physics to change the behavior of the student in the manner indicated by the writer's list of objectives. There is a good number of teachers who believe that the proper behavior objectives are achievable even without a conscious effort to teach for them. They assert that, for example, the ability to use the methods of science (objective 2) is acquired in a semiautomatic fashion by the students of any well-organized, clearly presented course in physics. Since these teachers do not as a rule consider it necessary to test for specific objectives, the validity of their assertion is difficult to evaluate. There have also been many reports of experimentation with teaching for specified objectives; the results were invariably claimed to favor this method of teaching. In many cases these beliefs were substantiated in quite an unambiguous manner by the results of tests. It may be suspected, however, that in some cases the results were influenced not only by the difference in method but also by the difference in the degree of effort and enthusiasm in the treatment of the experimental and the control groups. It is the writer's opinion, based on the above-mentioned reports and his own experience, that better results can be achieved by teaching consciously and specifically for the desired objectives.

Still another group of teachers consider the abilities included in objectives 2 and 3 as representing a mixture of information (objective 1) and cleverness. Since cleverness is not teachable, they argue, the only duty of the instructor is to supply the student with the necessary information. Without insisting on a good operational definition of cleverness or intelligence, one may freely admit that some students are brighter than others, and that, with other variables kept constant, they will do better in most tests. There are two lines of evidence, however, against the correctness or fruitfulness of analyzing all abilities into just two factors: the unchanging native abilities and the amount of information possessed. In the first place, results of tests show that students with apparently equal stores of information vary considerably in the pattern or profile of various abilities. Thus a student may be considerably above the average in ability 2.2 and lower than the average in ability 2.1. He may even show considerable variation relative to the group in presumably less dissimilar abilities 2.21 and 2.22. Similar evidence is afforded by fairly low correlation coefficients between various pairs of abilities as measured by paper-and-pencil types of tests. To account for this variation in the *kind* rather than merely degree of "cleverness," a psychological theory must postulate an inconveniently—and to the writer, unconvincingly—large number of the "vectors of the mind," to use Thurstone's terminology.

More direct evidence for the truth of the assertions that the abilities of students similar to those described under objectives 2 and 3, and even 4, can be improved by teaching, that the improvement cannot be accounted for by an increase in the store of information or on the basis of mere maturation, and that improvement is best obtained by teaching consciously and specifically for the desired objectives is contained in numerous reports on the subject.[2]

4. Testability. In order to answer the question of whether the degree of achievement of an objective is measurable, it is necessary to state the

[2] See Bibliography at the end of the article.

objective in terms of the students' behavior and to choose a test situation in which this behavior could be exhibited. Since the abilities, skills, and knowledge expected of a student are stated in a general language, a question arises as to whether such generalized abilities do exist. The assertion that, for example, the ability to apply empirical generalizations to specific phenomena exists, implies that in properly chosen test situations a given student will exhibit a behavior pattern that is to some extent independent of the choice of the principle and the phenomenon. If this condition is not satisfied, the sampling technique of testing is not applicable. The degree of independence from the content of an ability that can be improved by teaching must depend, of course, on the method of instruction. The writer has evidence that, under the teaching conditions prevailing at the University of Chicago, the ability described above does depend on the choice of the principle and the specific phenomenon. He therefore listed the ability under several headings:

1.21—the phenomenon is taught in class;
2.11—the principle is new and is stated;
2.21—the relation between the principle and the phenomenon is new and the situation is "academic;"
2.3 —the relation is new and the situation is "whole."

This last section is further subdivided according to the choice of principle. The language of the objective 2.21—or of any other objective—implies that the ability there stated does exist in the above sense. The evidence for that is the stronger the narrower the content; that is, one may be surer of the existence of the ability in the field of mechanics than in the whole of physics.

The classificatory scheme is not economical unless the abilities listed are independent of one another. This consideration has been the guiding principle in the writer's construction of the list of objectives. Under the conditions of teaching prevailing in the physical science courses at the University of Chicago and a few other institutions, the abilities 1, 2, and 3 are almost surely sufficiently independent of one another to make testing for each mandatory. The degree of independence seems to decrease as one goes to the second and the third decimals in the writer's classification, but the writer has less evidence to offer on this point. An obvious economy in teaching and testing is possible if one knows what degree of independence of each ability from other abilities and the content a given method of instruction is likely to produce. The absence of such knowledge makes a wider sampling of abilities and content advisable.

Even after it has been shown that some one factor (ability) is a strong determinant of the performance of students in a set of exercises, a very important question remains to be answered: whether this ability is properly described by the verbal statement, such as "ability to apply stated principle in physics?" This is the question of the *validity* of the set of exercises. An exercise is said to possess high "face validity" if most technically trained persons agree, upon inspecting the exercise, that it does test the ability specified. This test of validity should be usually supplemented with other tests. Among these are correlations of the performance of the student on the set of exercises in question with (1) his performance on tests of higher face validity, often in the form of essays, class discussions, or oral examinations; (2) his performance in subsequent courses; and (3) opinions of teachers. More direct tests of validity are now being experimented with at the University of Chicago. In these tests a student is presented with an exercise and is asked to "think aloud" as he works on the exercise. By analyzing the statements made by the students either spontaneously or in answer to questions from the person conducting the test it should be possible to determine the mental processes the student goes through in arriving at the correct answer.

There seems to be good agreement among those who have done work in the field of measuring various mental abilities and skills that valid tests of objectives similar to those in the writer's list are possible. The agreement on the less important question of the proper *form* of the test—whether the test must be in an essay form, or could be an "objective," that is, prepared-answer test—is less complete. This is especially true as regards testing for objective 4, proper attitudes and habits.

5. Comprehensiveness. The list of objectives should include all the important, desirable and realizable outcomes of instruction. The lack of comprehensiveness, both true and apparent, of

the writer's list stems at least partly from the purpose of the manual in which the list appears. The manual is intended to illustrate how the degree of achievement of various objectives could be measured. Therefore, only those objectives were explicitly stated for which appropriate test exercises were available. Thus under objective 4, no more than a sample of habits and attitudes is included. Further, since the manual contains only paper-and-pencil test exercises, the "phenomena," "facts" and "situation" are limited to verbal descriptions of these. This fact is reflected in the language of the objectives. This language could, however, be easily generalized to include the desired behavior of the student when confronted with real phenomena in the laboratory or the field.

Dangers in the Use of a List of Objectives

A list of objectives employs a scheme of classification and highly abbreviated symbolic representations—"descriptions"—of desirable student behavior. An agreement on the list on the part of a teaching staff involves compromise. Each member of the staff has his own system of classification—usually not explicitly stated—that is natural to him. He will not feel easy with the artificial scheme; he will find it difficult to incorporate into his teaching the objectives of this scheme or to give them proper emphasis. Also, because of the ambiguity of the symbolism involved, his ideas of the meaning of the objectives will differ, sometimes importantly, from those of his colleagues. Finally, a conscientious teacher will realize that, with his interpretation of the objectives, the list is not complete, that some very important outcomes of science teaching are not included. And, if the grade of the student is determined solely from measuring the degree of achievement in the objectives listed, the teacher is faced with a painful dilemma: whether to teach only those things for which the student is rewarded (by a grade), or also to include what to him seems important but for which the student will not be tested and thus cannot be immediately rewarded.

Teaching is an art and therefore for a full utilization of the teacher's talents and knowledge, a great degree of freedom should be allowed him in the choice of methods of instruction and of illustrative content. But teaching also is, or should be, a science. Therefore some degree of systematization is desirable. A teacher who feels unhappy with a given list of objectives should prepare one of his own and attempt a translation between the two lists. If he uses the language of behavior objectives, he will find in nine cases out of ten that the two lists cover the same ground. He could then use his own list of objectives and teach with a greater feeling of security than he had experienced before.

The difficulties and dangers described above would be even greater if a given staff accepted uncritically a list prepared by an outsider. The writer would feel more than adequately rewarded for his work if this paper should do no more than stimulate discussion of the possible uses of clearly stated objectives of various courses or curricula.

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Ph.D. Dissertation by Edward J. Furst, University of Chicago, Dec. 1948.

Science carries us into zones of speculation, where there is no habitable city for the mind of man.—ROBERT LOUIS STEVENSON.

Cooperative Testing Program*

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IN developing a satisfactory testing program for a course in physics it is well to keep in mind two things: (1) It is important that the testing program cover all the essential objectives of the course. (2) It is highly advisable that the nature of any one test in the program bear some resemblance to the set of specific achievements which it is designed to measure.

Not only do tests furnish the data, and frequently the only data, upon which student achievement is evaluated and grades are determined, but also these same tests determine to a very large extent what the student will study and how he will study it. Almost automatically the student's attention is focused on the *most important* material of the course, to wit, that material which is *covered in the tests*. If the tests exhibit gaps, it is likely that the students education will exhibit the same gaps. As an example, laboratory work in general physics, no matter how poor or how good, will never get the attention from the student that it deserves as long as satisfactory laboratory tests are missing in the testing program.

If it is important that a testing program cover all the essential objectives of a physics course, it is also important, I think, that the form of a test bear some resemblance to the set of achievements that it is designed to measure. In this respect paper and pencil tests are certainly necessary in any complete testing program but it is highly doubtful that they are sufficient. If the ability to devise and perform simple experiments in the laboratory is regarded as an essential objective in a physics course, then it is probably necessary to set the student to work in the laboratory in order to test this ability. Any other procedure seems highly artificial and is likely to give the student a false impression as to values.

What has just been said about the possible insufficiency of paper and pencil test applies all the more strongly to the so-called objective form of paper and pencil tests. It may be possible to construct an objective test for the measurement of any specified set of achievements in the field of physics but it becomes so difficult to do this in some instances that the net effect is that such tests are not constructed. Or, if they are constructed, they become highly artificial in the sense that they introduce a complex set of directions to be followed by the student which may be more difficult to analyze than the question to be answered. Is it really necessary or advisable to fit all tests to the Procrustean bed of the objective form? Procrustes procedure was fatal to a great many of his guests. Is it not equally fatal to a great many tests?

The papers presented in this symposium and the subsequent discussion clearly indicate that significant progress is being made in the construction of physics tests designed in terms of specific teaching objectives. That this development has occurred in certain localized systems of the AAPT assembly is also evident. This is as it should be, since a new development or idea has to take root in some one place before it becomes profitable to make a widespread distribution of it. There comes a time, however, when developments, such as we are discussing, should be made accessible to the whole assembly. Many of us think that that time has come with regard to the development and use of tests relative to objectives.

Proposals

For this reason, among others, the Committee on Tests and Testing of the AAPT submits the following proposals: That the AAPT sponsor a cooperative experimental testing program that will:

1. Seek information about the various physics testing programs now being developed in many educational institutions of this country.
2. Inform members of the AAPT, via the *American Journal of Physics*, about these testing programs.

* Remarks presented at symposium on "Testing in Physics" at the January (1949) meeting of the AAPT.

3. Arrange for a yearly conference on physics testing to be held as a part of the Annual Meeting of the AAPT.
4. Collaborate with the Educational Testing Service (formerly Co-operative Test Service) in the revision of the highly useful Cooperative Physics Tests.
5. Explore the feasibility of administering on a wide basis new types of tests in physics for the purpose of validation.

Mesons Old and New

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MESONS were invented by a Japanese physicist, Hideki Yukawa, in 1935. The existence of a beta-radioactive particle of mass intermediate between that of electrons and protons could have explained at once the occurrence of strong short-range nuclear forces, and some troublesome aspects of beta decay. So Yukawa postulated the existence of mesons, assuming them to interact strongly with nuclear particles. The subsequent discovery of just such particles in cosmic rays in 1937 is now an old story.

But this invention has not done as much as was originally hoped for. Physicists still do not understand nuclei. Theories that have been proposed to account for the behavior of mesons have been extremely ingenious, often mathematically elegant, and—at least quantitatively—unfruitful. Mesons provide a qualitative explanation for such things as the magnetic moment of the neutron and the electric quadrupole moment of the deuteron, but it is hard to think of a single quantitative result obtained so far from meson theories. Much has been learned about field theories by studying mesons, but not much about mesons by studying field theories.

One reason for this slow progress is that any meson theory of nuclear forces must be treated mathematically as a field theory—in fact, as a quantized field theory—in which mesons are thought of not as a prescribed number of balls suitably localized at each instant, but as waves extending through space and even changing in number. They are continually being created and absorbed. All meson theories of nuclear forces depend in fact on the supposed ability of nucleons to create and annihilate mesons. Quantized field theories provide the mathematical formulation of these properties, but such formulations are not always elementary and transparent.

Furthermore, one of the clearest things about nuclear forces is that they are *not* small. Yet in 1937 the only method that had been developed for handling quantized field theories was an approximate one (the perturbation theory with its imminent infinities) based on the assumption that the forces involved were small. Since that time a theory going to the other extreme of *strong coupling* has been successfully carried through—successfully in the sense that a very difficult mathematical problem has been stated and, by great ingenuity, solved. But this is also an approximation, and the truth would seem to lie somewhere between the weak-coupling and strong-coupling theories.

There is another very good reason for the lack of progress in meson theories—we did not know as much about mesons as we thought we did. Let us summarize the properties of mesons as they were known up to about three years ago.

Charge. Cloud-chamber tracks indicated that the charge is the same in magnitude as the fundamental electronic charge, and of either sign. Uncharged mesons, as yet undetected, are also required in most theories of nuclear forces.

Mass. The meson mass is about 200 times the mass of the electron. Probably the best experiments were those of Fretter,[1] who determined the magnetic curvature of many meson tracks and the range of the mesons in a pair of cloud chambers arranged one above the other. Fretter's value was 202 electron masses. (Obtaining reli-

[1] W. B. Fretter, *Physical Rev.* **70**, 625 (1946). See also discussions by H. A. Bethe, *Physical Rev.* **70**, 821 (1946), and D. J. Hughes, *Physical Rev.* **71**, 387 (1947).

able values of magnetic curvatures is not easy. Scattering by the gas particles in the cloud chamber can produce spurious curvatures. This is probably responsible for the wide variations in mass values found by various experimenters.[2])

Spin. This is still a pretty open question. If mesons are produced by nucleons in accord with Yukawa's idea, the spin must be integral (in units of $h/2\pi$)—unless, of course, mesons are produced in pairs. Some theories have been developed in which the spin is not unique, but mesons of both zero spin and spin one are needed to hold nuclei with suitable firmness. Spin is not a property that is simply and directly measurable.

Lifetime. Yukawa's hypothesis that the particles of intermediate mass should be radioactive has been verified experimentally in the cosmic-ray particles. A few cosmic-ray mesons have been photographed in cloud chambers in the act of decaying.[3] And to determine the life time, many of the decay electrons have been counted and even timed. Nereson and Rossi's value[4] of 2.15 microseconds for the mean life time is the result of a very clean determination, and the principle of the experiment is typical of several similar measurements. Essentially, some cosmic-ray mesons are slowed down and stopped (by collisions) in a solid absorber, and one measures how long it takes for the electron to come out after the meson went in. The times are observed to be distributed according to the usual exponential law for radioactivities. Of course, the counting and timing is all done by electronic circuits.

Following previous experimenters, Nereson and Rossi used a thick lead shield above their absorber to stop the soft cosmic radiation (electrons) and to slow down mesons (see Fig. 1). A collection of Geiger-Mueller tube counters, arranged in coincidence and anticoincidence circuits, recorded the arrival of a particle (meson) from above, departure of a particle (presumed to be the decay electron) sideways, and nothing coming out the bottom of the absorber. What was actually recorded each time this set of events occurred was an electrical pulse whose magnitude was a function of the time interval between firing of the counters above (entrance) and to the side (exit) of the absorber. Rossi and Nereson estimated that roughly one electron was produced for every two mesons that entered but did not leave their absorber. This ratio was to be anticipated, since only positive mesons would be expected to decay in this fashion in solid matter.[5] (The word "electrons" has been used repeatedly to include positrons as well as negatrons.)

Why the supposed difference in behavior between + and − mesons? Why do only positive mesons die a natural radioactive death? The answer is that nuclei are also positively charged. And mesons are supposed to interact strongly with nuclei. (This must be so, if mesons are to provide strong nuclear forces.) So, before they can decay, negative mesons are presumably captured by positive nuclei—and are murdered![6] Positive mesons are saved by the Coulomb repulsion from the nucleus, which keeps them a safe distance away.

This, then, is a rough description of our knowledge of mesons in 1945. Recently there has been a series of very striking and upsetting experiments—striking enough in some cases to be reported in the daily papers.

Decay of Negative Mesons

The first of these new experiments was performed in Rome by Conversi, Pancini and Piccioni,[7] and was very similar to previous work on meson lifetimes. As usual, something new had been added—a method of selecting for the experiments mesons of either sign. The ingenious selector was a "magnetic lens," which replaced the lead shield of earlier experiments. The "lens" consisted of a pair of iron plates, magnetized in the plane of the plates, and placed face to face, with opposite directions of magnetization. Thus if, in Fig. 2, the right-hand plate of a pair of plates has its direction of magnetization out from the paper, and the left-hand plate has its direction of magnetization into the paper, the magnetic field in each plate will bend positive particles towards the central plane between the

[2] H. A. Bethe, *Physical Rev.* **70**, 821 (1946).
[3] E. J. Williams and G. E. Roberts, *Nature* **145**, 102 (1940). R. P. Shutt, S. De Benedetti and T. H. Johnson, *Physical Rev.* **62**, 552 (1942).
[4] N. Nereson and B. Rossi, *Physical Rev.* **64**, 199 (1943).
[5] S. Tomonaga and G. Araki, *Physical Rev.* **58**, 90 (1940).
[6] Professor Edward Teller suggested this lurid description at a colloquium at Iowa State College.
[7] M. Conversi, E. Pancini and O. Piccioni, *Physical Rev.* **71**, 209 (1947).

plates and will deflect negative particles away. The plates can of course be reversed to select negative particles instead of positives.

The experiment consisted in selecting particles of either sign, and counting the number of threefold (ABC) and fourfold ($ABCD$) coincidences. A charged particle that tripped an A-counter and the corresponding B-counter, but not a D-counter below the absorber must have stopped in the absorber. (The decay electrons tripped the C-counters. The lead shield below the C-counters, without affecting penetrating mesons, absorbed any electrons which might otherwise have tripped the D-counters.)

With this equipment, the Italian physicists hoped to test the supposedly different behavior of the positive and negative mesons when brought to rest. And when the absorber used was iron, the results verified beautifully the expectations. When positive mesons were studied, the number of decay electrons counted was 67 per hundred hours—a robust positive number. The corresponding number of negative mesons was 3 per hundred hours. The experiment was repeated with a graphite absorber, and with *completely different results*. In this case, almost as many decay electrons were observed from negative mesons as from positives. Negative mesons are not murdered in carbon but are allowed to die a natural death!

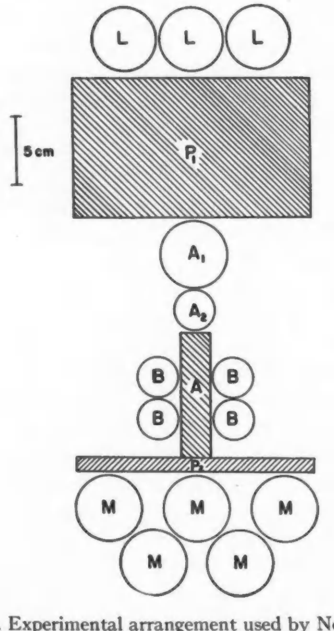

FIG. 1. Experimental arrangement used by Nereson and Rossi for determining the rate of decay of mesons. A lead shield P_1 is used to absorb the cosmic-ray electrons. An auxiliary lead plate P_2 is used to shield counters M from decay electrons. If a meson enters the apparatus from above, and is absorbed in the absorber A, it will discharge one of the counters L and counters A_1 and A_2, but will not discharge any of the counters M. The counters B record the decay electrons.

This may not sound so remarkable. After all, experiments have given surprising results before. In fact, if they didn't, there would be little interest in performing them. But this was really a result of prodigious significance. Fermi, Teller and Weisskopf[8] estimated the length of time that should be required for a negative meson to be captured by a nucleus, and came to the conclusion that if the experiments of Conversi, Pancini and Piccioni were verified, this would indicate a discrepancy from theory by a factor of about 10^{10}—a significant discrepancy even to theorists.

Yet these experiments have now been repeated in one form or another in many laboratories.[9] And mesons seem to behave—or rather to misbehave—just as the Rome group said they do.

The story of meson mortality seems to be as follows: Positive mesons decay by a single process in which positrons are emitted. This is the process with a mean lifetime of 2.15 microseconds. Negative mesons can be removed from this world by either of two competing processes. One of these is radioactive decay, similar to the decay of the positives, and with the same decay constant. The other process is capture by a nucleus, and the probability of this process varies as the fourth power of the charge of the capturing nucleus.[10] This variation of rate of capture with atomic number explains why decay of electrons is *not* observed in material of atomic

[8] E. Fermi, E. Teller, and V. Weisskopf, *Physical Rev.* **71**, 314 (1947). E. Fermi and E. Teller, *Physical Rev.* **72**, 399 (1947). The correctness of this calculation has recently been questioned. Cf. H. Fröhlich, R. Huby, R. Kolodziejski, and R. L. Rosenberg, *Nature* **162**, 450 (1948).

[9] T. Sigurgeirsson and A. Yamakawa at Princeton, *Physical Rev.* **71**, 319 (1947). G. E. Valley at M.I.T., *Physical Rev.* **72**, 772 (1947). (Valley had already started his experiments, using cloud chamber pictures to determine the sign of the charge, before the Italian experiments were reported.) G. E. Valley and B. Rossi, *Physical Rev.* **73**, 177 (1948). H. K. Ticho and M. Schein at Chicago, *Physical Rev.* **73**, 81 (1948). N. Nereson at New Mexico, *Physical Rev.* **73**, 565 (1948).

[10] J. A. Wheeler, *Physical Rev.* **71**, 320 (1947). H. K. Ticho and M. Schein, *Physical Rev.* **73**, 81 (1948).

number greater than about 15. But the puzzle remains why it is possible to observe the *decay* of negative mesons at all. A meson at rest could sit around for a couple of microseconds (on the average) before decaying. And it should take only about 10^{-12} seconds for a negative meson to be brought to rest in solid matter from an energy of 2000 ev, and to be captured into a nucleus.[8] (Of this time, the capture process itself was estimated to require only 10^{-18} seconds in carbon.)

As a matter of fact, this question—which was raised in so sharp a manner by the Rome experiment—was not wholly new. Cosmic-ray experiments had long ago suggested that between nuclei and mesons passing through matter, the interaction was embarrassingly small when compared to the magnitude of nuclear forces supposedly produced by mesons, or to the ease of production of mesons by cosmic-ray primary particles.[11] But these earlier troubles were minor discomforts compared to a factor 10^{10}.

These matters were discussed at a physics conference at Shelter Island, New York, in June 1947, where Marshak suggested that perhaps there were in fact two different types of mesons. One of these would interact strongly with nucleons, and thus be readily produced in the highly excited nuclei resulting from high altitude cosmic-ray impacts. This primary meson would then decay into a secondary meson—the one observed in sea-level experiments—which must be a very shy type of particle, exhibiting virtually no (nonelectric) direct interaction with nuclear particles. Before a paper by Marshak and Bethe,[12] describing and developing this hypothesis, had been completed, news of a second striking but beautifully simple experiment reached this country.

This new experiment, performed by Lattes, Muirhead, Occhialini, and Powell of the University of Bristol, consisted in sending some photographic plates to an observatory (Pic du Midi) in the Pyrenees mountains, leaving them there (under very thin covering) "exposed" to cosmic rays for a few weeks, then bringing them back to the laboratory, developing them and

[11] L. W. Nordheim and M. H. Hebb, *Physical Rev.* **56**, 494 (1939).

[12] R. E. Marshak and H. A. Bethe, *Physical Rev.* **72**, 506 (1947).

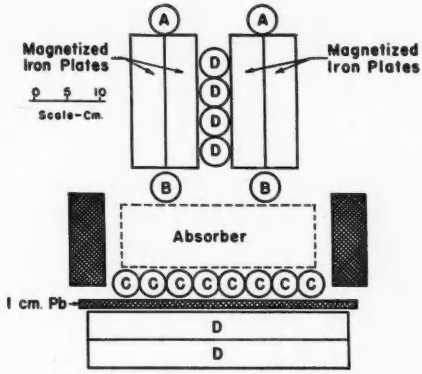

FIG. 2. Experimental arrangement used by Conversi, Pancini and Piccioni for studying the decay of positive and negative mesons. The lead shield of Nereson and Rossi has been replaced by pairs of magnetized iron plates. These concentrate mesons of one sign towards a counter *B*. Mesons of opposite sign are deflected away from the counter *B* and do not register in the experiment. Counters *A* and *B* indicated incoming mesons; counters *C* counted the decay electrons.

looking at them. But this description is an enormous understatement. Actually the experiment was a consequence of the careful work, over several years, by the group working with Dr. Powell—developing and improving, until it has become almost a new instrument, the old technique of using photographic plates for nuclear and cosmic-ray recording.

Nuclear Photographic Emulsions

Alpha-particle tracks were produced and observed in photographic plates as early as 1912. The general recognition of the possibilities in the use of plates as recorders of nuclear particles seems to have developed during the 1930's. As an example of the prewar contribution of photographic plates to our cosmic-ray and nuclear understanding, we might cite the discovery of cosmic-ray "stars" by Blau and Wambacher in 1937.[13] The name "star" is of course a description of the microscopic picture left in an emulsion

[13] M. Blau and H. Wambacher, *Nature* **140**, 585 (1937). For a general survey of the use of the photographic method, see the review articles of M. Shapiro, *Rev. Mod. Physics* **13**, 58 (1941) or C. F. Powell, *Proc. Roy. Soc.* **A181**, 344 (1943). For a discussion of the recent improvements, see C. F. Powell, G. P. S. Occhialini, D. L. Livesey and L. V. Chilton, *J. Sci. Inst.* **23**, 102 (1946) or B. T. Feld, "The Photogenic Mesons," Technical Report No. 8, Laboratory for Nuclear Science and Engineering, Massachusetts Institute of Technology.

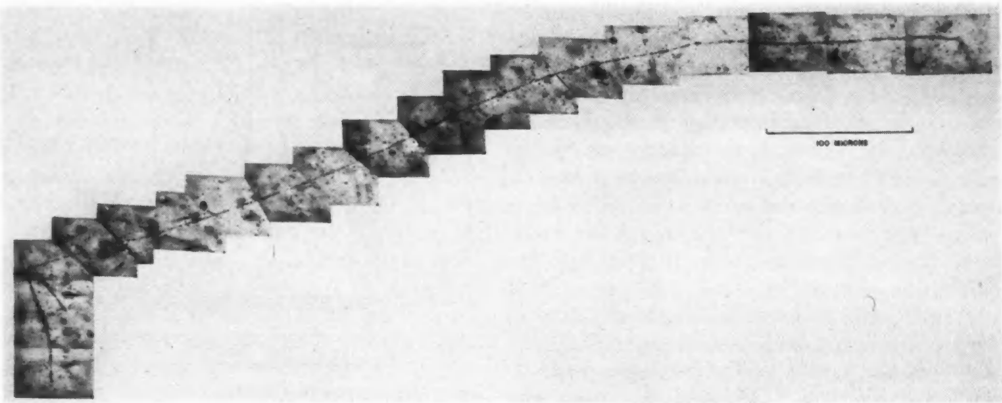

FIG. 3. A $\pi-\mu$ decay. The track at the extreme left side is that of a π-meson. This particle stopped in the emulsion, giving rise to a secondary μ-meson, which left the long track shown crossing the figure to the right. Note on both tracks the continuous curvature and the rapid increase of grain density toward the (low velocity) end of the track, both characteristic of meson tracks. The figure is made up of a series of photomicrographs. This π-meson was produced in the Berkeley synchrocyclotron.

when a cosmic-ray particle disintegrates one of the nuclei from the emulsion.

The advantage of recording a particle track in a photographic plate or in a cloud chamber, as opposed to counting it in a counter or ionization chamber is obvious. The suggestion is illuminating, though not new, to consider a photographic plate as a continuously sensitive cloud chamber which happens to have, among other properties, the relatively high density characteristic of a solid. This means a correspondingly increased chance of slowing down and stopping particles within the emulsion—an incalculable advantage for cosmic-ray work, and surely also for nuclear research in the billion volt region. It also means that observation must be by high-power microscopes, since the track lengths of interest will be from perhaps a few microns to a few hundred microns. The time required for the solid emulsion to slow down and stop a particle is also proportionally less than the time the atmosphere takes to do it—perhaps by a factor of 2000. So there is a chance for radioactive particles that would decay in flight in air to be stopped in an emulsion before the decay occurs. Advantages of photographic plates over the Wilson cloud chamber, as regards weight and cost, are again obvious.

The drawbacks of prewar emulsions lay in the *fog* or background of silver grains, and in the relatively large grain spacing. Large grain spacing means tracks difficult to follow—especially so when produced by lightly ionizing particles—and tracks with uncertain ends. The nuclear physicists at Bristol and the research workers in the laboratory of the photographic firm of Ilford, Ltd., worked together, studied these limitations, and have overcome them. The outcome of this cooperation is the new Ilford "nuclear research" emulsion, containing eight times as much silver halide as prewar emulsions, and distributed in finer grains. Not only has the grain spacing been materially reduced, but the smaller grain size increases the resolution between tracks produced by different kinds of particles. The new plates record tracks of mesons of energies up to about 10 Mev.

π- and μ-Mesons

Using the Ilford plates, the Bristol group[14] had learned to identify meson tracks by their very low grain density, by the rather rapid rate of increase in grain density along the track (as the mesons are slowed down), and by the almost continuous changes in direction of the track due to Coulomb scattering by nuclei of the emulsion. In continuing their observations of tracks left in photographic emulsions by cosmic rays, they

[14] G. P. S. Occhialini and C. F. Powell, *Nature* **159**, 186 (1947).

studied a number of tracks produced by mesons that stopped in the emulsion.[15] Of 65 such tracks, the ends of two of them marked the beginning of secondary tracks, which were definitely identified also as meson tracks. It should be pointed out that, because of the great increases in grain density near the end of a track, there was no uncertainty as to the direction in which the various mesons were heading. Nor was there any possibility that the apparent ends of the primary tracks and the beginning of the secondaries could, in fact, be elastic nuclear scatterings. Lattes and company considered the possibility that energy for the secondaries was provided by local nuclei, but it seemed much more probable that the process was one of radioactive decay of a heavier meson into a lighter meson and a neutral particle.[16] As to the relative masses of primary and secondary mesons, information obtained on the basis of the density of grains along the track, and the range of the tracks, was not conclusive when Lattes and co-workers published their first paper (May, 1947).

It has turned out that these decay processes, in which a primary meson essentially at rest gives rise to a secondary meson, are relatively frequent events. In a second paper,[17] the Bristol group included data from a set of plates exposed to cosmic radiation at Chacaltaya in the Bolivian Andes (altitude 18,000 ft). Their plates included 40 "pictures" of the process, in which primary (π) mesons are "seen" to decay into secondary (μ) mesons. Eleven of the secondaries remained completely within the emulsion, leaving developable tracks for their entire range. And while the direction of emission of these secondaries was apparently random, the secondaries all had almost exactly the same range—614 ± 8 microns. Thus all the μ-mesons must have been produced with the same energy, and this almost certainly implies that the π-meson breaks up on decay into exactly two particles. The two particles must have equal and opposite momenta, and must share between them the rest energy of the π-meson. One of the decay products is the μ-meson, which must carry the same charge as

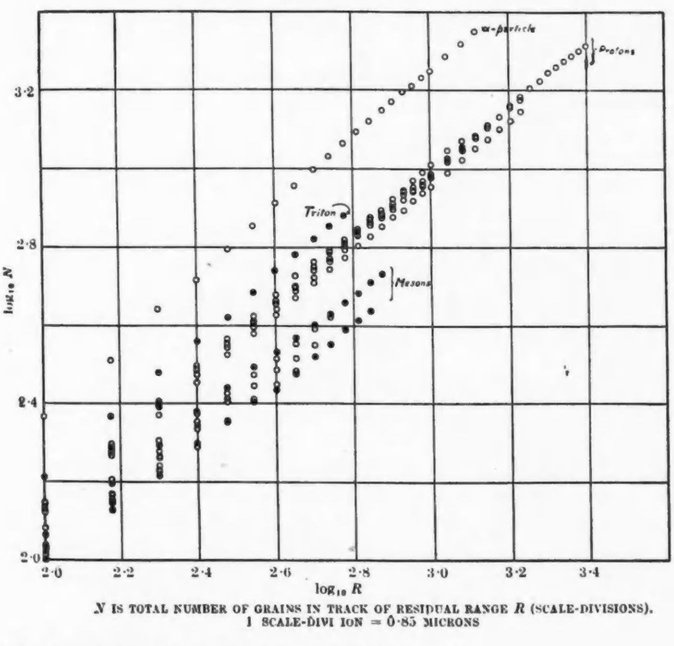

FIG. 4. (From C. M. G. Lattes, G. P. S. Occhialini and C. F. Powell, *Nature* **160**, 453 (1947).) These data were taken from the tracks in a single plate. The "spread" is attributed to different degrees of fading in the emulsion during a six-week exposure.

N IS TOTAL NUMBER OF GRAINS IN TRACK OF RESIDUAL RANGE R (SCALE-DIVISIONS). 1 SCALE-DIVI ION = 0·85 MICRONS

[15] C. M. G. Lattes, H. Muirhead, G. P. S. Occhialini and C. F. Powell, *Nature* **159**, 694 (1947).
[16] F. C. Frank has looked carefully into various alternative hypotheses, and concludes that none are possible interpretations of the tracks observed. F. C. Frank, *Nature* **160**, 525 (1947).
[17] C. M. G. Lattes, G. P. S. Occhialini and C. F. Powell, *Nature* **160**, 453 and 486 (1947).

the original meson. The other secondary must be neutral, since it leaves no track. Figure 3 shows a $\pi-\mu$ decay of a meson produced in the Berkeley synchrocyclotron.

Mass Determinations

What can one say about the masses of the two types of mesons? The masses can be estimated by something like a range-energy relation. (The mean distance a charged particle will travel through matter before it stops is related to the kinetic energy of the particle in a definite way, which depends on the mass of the particle.) The procedure adopted by Lattes, Occhialini, and Powell was to count the number of grains in every 50 microns of track, and then plot (on log-log paper) against residual range of track the total number of grains in this residual range (see Fig. 4). In a rather rough way, the number of developed grains is a measure of the kinetic energy of a particle, just as in a cloud chamber, the number of droplets to the end of a track (and hence the number of ion pairs) is a measure of the kinetic energy of the ionizing particle. A set of calibrations can be obtained by comparison with protons, etc. But the Bristol physicists have been careful to point out the limitations of this method due to fading of the latent image with time, and also due to fluctuations in density of the original silver halide grains in the emulsion.

However, for a mother-daughter pair of π- and μ-mesons, the effect of fading should be the same on both mesons. And Lattes, Occhialini, and Powell developed a clever method of estimating the ratio of masses of π- to μ-mesons, based essentially on the ratio of grain densities in the primary and secondary track.[18] The argument is as follows:

> The rate at which a moving charged particle loses energy in electronic collisions is

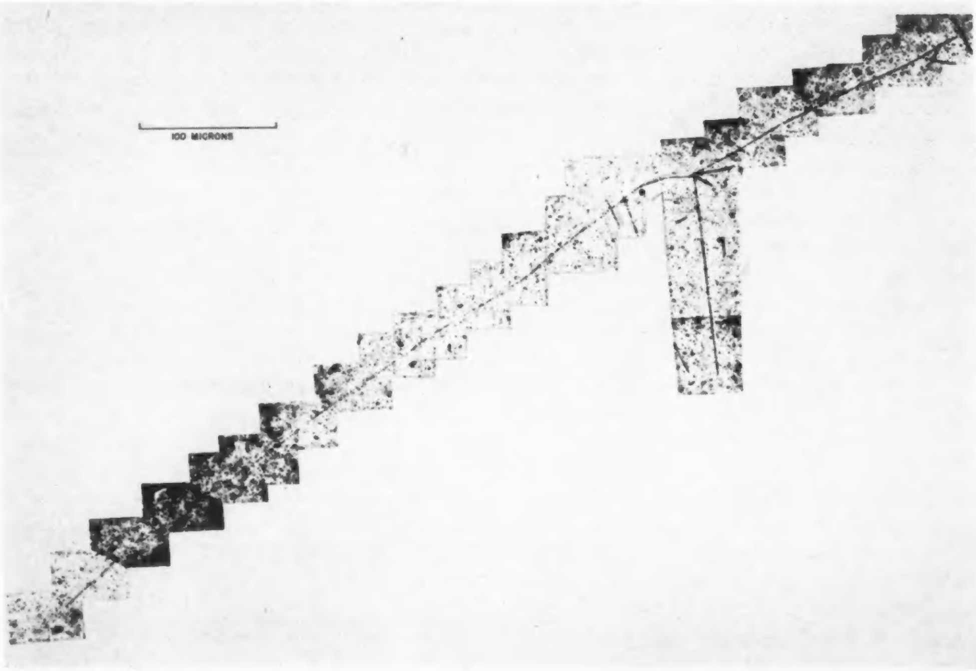

FIG. 5. Photomicrographs of a star produced by a Berkeley meson. The meson track, starting from the lower left-hand corner of the figure, shows the characteristic wobbles, and a grain density initially low but rapidly increasing near the star, as the meson was slowed down.

[18] C. M. G. Lattes, G. P. S. Occhialini, and C. F. Powell, *Proc. Physical Soc.* **61**, 173 (1948).

proportional to the square of the charge carried by the moving particle,[19] and is also a function of the velocity of the particle. (The rate of losing energy is not separately dependent on the energy of the particle, or on its mass, but only on the ratio of energy to mass.) In general (except for very high velocities), the less the velocity, the greater the energy transfer from a moving particle to electrons of the material. All this is predicted by the elementary theory of the slowing down of charged particles, and is familiar from cloud-chamber tracks, etc. Thus the reason that protons make heavy tracks in cloud chambers, while electrons characteristically leave thin spotty tracks is that the heavy particles are travelling at much lower velocities.

Since velocity is immediately calculable from the ratio of energy to mass, the energy loss per unit length of path, or the rate of decrease of energy with residual range, can be written as

$$dE/dR=[d(E/m)/d(R/m)]=f(E/m).$$

One concludes from this that, for singly charged particles, there is a universal relation of the form $E/m=\phi(R/m)$. Lattes, Occhialini, and Powell assumed that the slowing down process is the same in an emulsion as in a gas, and further that the number of developed grains per path length (dN/dR) depends only on the loss of energy of the particle over the same path. So they concluded that, for any mass

$$N=mF(R/m).$$

Applied to π- and μ-mesons, this equation says that a curve of $\log N$ plotted against $\log R$ for the π-mesons should be identical in shape with the corresponding curve for the μ-mesons, only displaced from the latter in both abscissae and ordinates by the log of the mass ratio. By measuring the displacement of the two experimental curves, Lattes, Occhialini, and Powell obtained for the ratio of masses of primary and secondary mesons $m_\pi/m_\mu=1.65\pm0.11$.

These experiments carried out in Bristol were, as we have seen, extremely timely. Just when theorists were being forced to the conclusion that no single particle could behave as badly as the mesons of Conversi, Pancini, and Piccioni, Powell and his co-workers showed conclusively that there are in fact at least two different types of mesons, with different masses. It is reasonable to expect that they differ in other properties as well. Most of the experimental inconsistencies disappear if one assumes, as did Marshak and Bethe,[12] that the primary (π) mesons interact strongly with nuclei, but that they last only for short times. The secondary, or ordinary (μ) mesons make abundant cloud-chamber tracks at sea level, but have very little interest in nuclei. Conversi, Pancini, and Piccioni perversely performed their experiment at the bottom of our atmosphere, where the mesons have all become μ's. The same statement holds, presumably, for the mass determinations of Fretter, the lifetime measurements of Rossi and Nereson, and so on.

Nothing has been said so far about the other particle that must be produced in a $\pi-\mu$ decay. The neutral particle or *neutretto* appeared at one time to be still a third type of meson. For though it leaves no track, its mass can be determined. This follows from the conservation of energy and momentum in the decay process. Once the mass of the μ-meson is known, its energy can be determined from its range. Knowing the total energy (the rest-mass energy of the π-meson) available for the process, and the energy and momentum carried off by the μ-meson, properties of the unseen partner are determined. The mass originally calculated by the Bristol group for the neutretto was 115 ± 30 electron masses.

σ- and ρ-Mesons

The new photographic techniques have been used for other studies of meson properties, and one of these is the relation of mesons to stars. As was mentioned before, the production of stars in photographic emulsions (by explosions of nuclei) had been known for some time. In the plates exposed to cosmic rays by the Bristol team, many examples of stars were found. The pictures of particular interest were those in which mesons could be seen entering or leaving the stars. The former illustrate the death of mesons by nuclear capture, the latter their creation from out of

[19] A study of grain density near the ends of meson tracks indicated that the mesonic charge has the same magnitude as that of the electron. This was subsequently verified in other ways. See references 20 and 24.

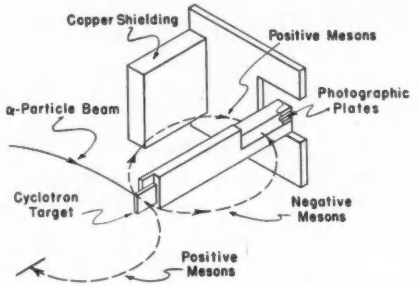

FIG. 6. Some geometric arrangements used in the Berkeley cyclotron to photograph mesons. The alpha-particle beam striking the target ejects mesons in all directions. Negative mesons knocked forward are deflected by the magnetic field away from the alpha-beam and into photographic plates. Positive mesons may be observed nearer the center of the cyclotron (and above or below the plane of the alpha-particles), or in the same plates used to record the negative mesons.

nuclei. This led to a further distinction between mesons, entirely separate from the previous $\pi-\mu$ classification. Mesons that produce stars are designated as σ (star-producing) mesons. There are other mesons that quietly come to the end of their range and disappear; these are called ρ-mesons. The $\rho-\sigma$ distinction is a purely phenomenological one. Figure 5 is a photomicrograph of a meson-induced star.

It is tempting to speculate, as Lattes, Occhialini, and Powell did, that most of the σ-mesons are similar to π-mesons—probably the negatively charged of the species. For if heavy mesons of both signs exist, all negatives should be captured by nuclei at the end of their lives, while the positives would be protected by electrostatic repulsion long enough to decay into positive μ-mesons. (This for behavior in a solid. In empty space, the negatives can also decay.) Likewise, most of the light μ-mesons should appear as ρ's.

Goldschmidt-Clermont, King, Muirhead, and Ritson,[20] working at Bristol, developed a method for determining masses, based on the frequency and magnitude of random small-angle scatterings due to electrostatic forces. One of the characteristic features of meson tracks is their wandering nature. The mesons, of small mass, are easily knocked about, and how easily is an indication of how small the mass. The method, since it is statistical, is less accurate than the best grain-counting techniques, but it is not seriously impaired by fading of the track. In this experiment, mesons were classified as π or μ if the other member of the pair could be surely identified. Mesons tracks ending within the emulsion were further classified as ρ and σ. Results from 20 π- and 20 μ-mesons, and from 60 σ- and 60 ρ-mesons were (in terms of the electron mass m_e)

Mass of π-mesons	$260\pm30\ m_e$
Mass of μ-mesons	205 ± 20
Mass of σ-mesons	275 ± 15
Mass of ρ-mesons	200 ± 10

These results agree with the experiments and predictions of Lattes *et al.* within experimental error.

If only heavy mesons interact strongly with nuclei, then the mesons produced in stars should also be predominantly heavy. Evidence for this is growing. In a paper published in July, 1948,[21] Occhialini and Powell reported that they obtained photographs of twenty nuclear explosions in which a meson was produced. Seventeen of the mesons were σ's. It is rather strange that Occhialini and Powell have observed no π-mesons ejected from their stars. But they point out that positive mesons ejected from nuclei should start out with a large kinetic energy, due to the electrostatic repulsion by the nucleus. It should be remembered that only low energy mesons were observable in the photographic plates used.[22]

The ultimate fate of the ρ-mesons has been very unclear. Only within the last few months has a reasonable explanation been proposed for the mysterious disappearance of these undemonstrative particles.

Artificial Mesons

If it is true that the forceful mesons have a mass half again as large as was previously expected, then the energy needed for the production of man-made mesons is increased accordingly. The discovery of the heavy meson explained

[20] Y. Goldschmidt-Clermont, D. T. King, H. Muirhead and D. M. Ritson, *Proc. Physical Soc.* **61**, 183 (1948).

[21] G. P. S. Occhialini and C. F. Powell, *Nature* **162**, 168 (1948).

[22] W. H. Barkas, *Physical Rev.* **75**, 1467 (1949), reports a similar result with cyclotron-produced mesons. Studying production of mesons in an energy range from 2 to 5 Mev, he reports that "The ratio of positive to negative mesons, which is about 1/5 for carbon, falls virtually to zero for heavy elements."

why, for instance, no mesons had been obtained from 100-Mev betatrons. For in order to produce a free meson, the energy corresponding to its rest mass must be supplied. According to Einstein's well known relation—$E=mc^2$—this energy is proportional to the rest mass. For electrons the rest energy is 0.51 Mev. A (light) cosmic-ray meson, which is two hundred times as heavy as an electron, would require just over 100 Mev. If heavy mesons (of mass 330 electron masses) must be produced, the energy required would be 168 Mev. The only existing machine that might possibly have been tough enough for the job was the Berkeley synchrocyclotron. And in March of last year, Gardner and Lattes[23] reported the first detection of artificial mesons.

As a matter of fact, the big cyclotron did not have very much to spare. Presumably the energy for meson creation—about 160 Mev—must be concentrated in a single nucleon (proton or neutron). The Berkeley synchrocyclotron can accelerate alpha particles to about 380 Mev, or 95 Mev per nucleon. But fortunately there is available some vibrational energy of the constituents of the target nuclei and the bombarding alpha-particles, and this is enough to make the process go.

Even so, the job of discovering the mesons was not by any means a simple one. People looked for them in such obvious places as the neutron beam (produced by bombardment of a target by charged particles) or in the alpha-beam itself, until it became clear that background made these searches hopeless. Photographic plates containing meson tracks were eventually obtained, by placing the plates near the cyclotron target, but outside of the beam. Negative mesons produced by the alpha-particles striking the (carbon) target were bent away from the heavy particle beam by the magnetic field of the cyclotron, and entered the plates edge on. (See Fig. 6.) Location of these tracks in the developed plates—no trivial problem in itself—and their identification as meson tracks was greatly aided by Lattes' experience in the techniques developed at Bristol.

The production of mesons in the laboratory was of course a triumph in itself. It is important to know that one can make mesons. But it is even more important that there is now a source of mesons for study and experimental purposes. The Berkeley physicists are working hard to learn the properties of their artificial mesons—the masses, lifetimes, excitation functions, types of nuclear interactions, and so on. Many of their results are still in the "grapevine" stage.

The Berkeley cyclotron makes both heavy and light mesons, and of both signs. One question of interest is whether any light mesons are produced directly by the accelerated particles, or whether they are made only by decay of the heavy mesons. The answer seems to be that *all* light mesons are secondary particles.[24]

Production of the mesons in the magnetic field of the cyclotron is an ideal way of separating positive and negative mesons, and of measuring their momenta. Negatives, as already mentioned, are photographed in plates located outside of the cyclotron beam. The positive mesons are caught either on plates located inside of the alpha-particle orbit of the cyclotron (and below the plane of the alpha-beam), or simply on the other edge of the plates used to trap the negatives (Fig. 6).

From knowledge of the momentum of a meson (the curvature that must have existed in the magnetic field provides this), and from the range of the meson in the photographic emulsion, the energy and the mass of the meson can be obtained. Masses can also be checked by grain counting methods. The result for the heavy meson, as quoted by Lattes and Gardner at the Washington meeting of the Physical Society[25] in April, 1948, was 313 ± 16 electron masses.

More recent work in Berkeley on the meson masses has greatly improved the accuracy with which these quantities are known. Present values[26] for masses are

$$m_\pi=285\ m_e,$$
$$m_\mu=216\ m_e,$$
$$m_\pi/m_\mu=1.32\pm0.01.$$

As the accuracy has gone up, the ratio of the masses of π- and μ-mesons has come down. This

[23] E. M. Gardner and C. M. G. Lattes, *Science* **107**, 270 (1948).

[24] C. M. G. Lattes, *Physical Rev.* **75**, 1468 (1949).

[25] C. M. G. Lattes and E. Gardner, *Physical Rev.* **74**, 1236 (1948).

[26] A. S. Bishop, *Physical Rev.* **75**, 1468 (1949). I understand that recent Bristol measurements are in essential agreement with these results.

requires a revised estimate for the mass of the neutretto—the hypothetical neutral particle supposed to be produced in the $\pi-\mu$ decay process. The observed ratio of π to μ masses is now just small enough to allow this neutral particle to have zero mass. Instead of being another kind of meson, it would now appear to be a photon or neutrino. This is rather satisfying, unless one is not yet reconciled to living with neutrinos.

Determination of the rate of $\pi-\mu$ decay of negative π-mesons was another accomplishment of the California group.[27] The π^--mesons would be expected to make nuclear disintegrations in solid material, but in the vacuum of the cyclotron tank some of them should decay to negative μ's. To determine the rate of decay mesons projected forward from the cyclotron target were selected by spiral channels. One channel, rising above the plane of the cyclotron alpha-particle beam, guided mesons into photographic plates after they had completed 180° (one half turn) of their orbit in the magnetic field. Another channel, spiralling below the plane of the beam, allowed mesons to go through 540° (a complete turn and a half) before being photographed. The number of heavy mesons that "dropped out" in the extra lap provided a measure of the decay rate. Richardson found for the mean life, 1.11×10^{-8} second.

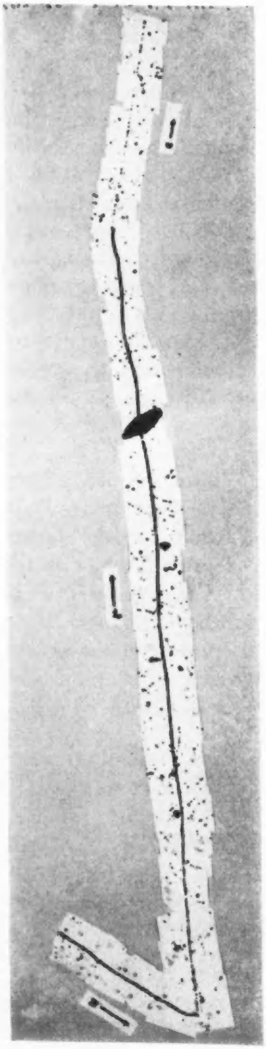

FIG. 7. This remarkable picture, from C. F. Powell's laboratory in Bristol, shows the complete life cycle of a μ-meson: its production from a primary π (at the bottom of the figure), and its eventual decay into an electron (and neutral particles) near the top. The emulsion used was a new type designed to be sensitive to electrons.

Decay of ρ- (or μ-) Mesons

What happens to ρ-mesons at the end of their paths? If they are the light cosmic-ray mesons studied before 1947, then presumably the positives decay into positrons and neutrinos. The negatives would be expected to be captured by heavy nuclei. In the capture process, the nuclei must temporarily become excited by 100 Mev (the rest energy of light mesons), and a fever like this should show itself in all kinds of eruptions. But no eruptions are seen at the ends of ρ-meson tracks. At one time it was thought that the neutretto had something to do with this.

Some preliminary evidence that the neutretto was produced in the decay of light mesons was claimed from two cloud chamber pictures (out of a set of 9000) taken in a B-29 at 30,000 ft by Anderson, Adams, Lloyd, Rau, and Saxena.[28] The cloud chamber was in a magnetic field of 7500 gauss. Both pictures show the track of a positron of order 25 Mev starting from the end of the track of an incoming particle. In one of these pictures the incoming particle is clearly identified as a meson, though of uncertain mass. In the second picture, the primary is *probably* a meson. Assuming that these pictures really indicate a two-particle decay, the other particle

[27] J. R. Richardson, *Physical Rev.* **74**, 1720 (1948).
[28] C. D. Anderson, R. V. Adams, P. E. Lloyd and R. R. Rau, *Physical Rev.* **72**, 724 (1947). R. V. Adams, C. D. Anderson, P. E. Lloyd, R. R. Rau and R. C. Saxena, *Rev. Mod. Physics* **20**, 334 (1948).

would have a mass of approximately 140 electron masses. This mass agreed with early values for the neutretto mass (115 ± 30 electron masses).

But even if some light mesons decayed in this fashion, it appeared that this was not the only way. Hincks and Pontecorvo at Chalk River reported an experiment[29] on the absorption of the secondary electrons from meson disintegration. They concluded that the maximum range of electrons observed was too great for the electron to have a maximum energy of only 25 Mev, but was in accord with a maximum energy of 50 Mev, as required for meson decay into a positron and neutrino.

Recently, Anderson and co-workers[30] at Pasadena have reported experiments in which they have obtained cloud chamber pictures at sea level in which over 50 electrons have been observed from the decay of cosmic ray mesons. The decay electrons have energies ranging from about 10 Mev to about 50 Mev. This is fairly clear evidence for the production of three or more particles whenever a μ-meson decays, permitting a variety of modes of decay and a range of secondary electron energies. Figure 7 shows the decay of the μ-meson into an electron and neutral particles.

Current Interpretations

Based on some arguments concerning the spins of mesons, Serber[31] has suggested that the light positive meson decays into an electron and *two* neutrinos. Likewise a neutrino is again invoked to carry away the excess energy when a light negative is captured by a nucleus. If Serber's identification of the meson spins is correct, a neutrino is required to conserve angular momentum in the capture process, as well as in the decay of a free light meson. This scheme has the virtue of attributing the disappearance of energy whenever it occurs—in decay of π-mesons, decay or capture of μ-mesons, or in beta decay of nuclei—to one single energy thief.

Serber's scheme for meson spins and interactions seems in at least qualitative accord with all experiments. It can be summarized as follows:

1. Nuclear forces arise out of the creation and annihilation of π-mesons by nucleons. Protons (P) and neutrons (N) "exchange" π-mesons. This requires an integral spin (in units of $h/2\pi$) for π-mesons, as in Yukawa's original hypothesis. A typical "fundamental process" would be

$$P \rightarrow N + \pi^+.$$

2. A π-meson can decay into a μ-meson (of the same sign of charge) and a neutrino (ν). Since a neutrino is assumed to have a spin of 1/2 unit, conservation of angular momentum requires that the μ-meson also have a spin of 1/2. Such a decay is summarized by the symbols

$$\pi^\pm \rightarrow \mu^\pm + \nu.$$

3. In free space a μ-meson decays into an electron (e) and two neutrinos according to the scheme

$$\mu^\pm \rightarrow e^\pm + 2\nu.$$

4. A μ-meson can be captured by a nucleon, ejecting a neutrino in the process. To describe this reaction we write

$$\mu^- + P \rightarrow N + \nu.$$

I am very grateful to Dr. E. Gardner and others at Berkeley, and to Dr. C. F. Powell of Bristol for providing the photomicrographs and for other assistance in preparing this article.

[29] E. P. Hincks and B. Pontecorvo, *Physical Rev.* **74**, 697 (1948).
[30] C. Anderson, R. Leighton, and A. Seriff, *Physical Rev.* **75**, 1466 (1949).
[31] R. Serber, *Physical Rev.* **75**, 1459 (1949).

The thinker makes a great mistake when he asks after cause and effect. They both together make up the indivisible phenomenon.—GOETHE.

Oil Well Logging—An Opportune Field for the Physicist

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THE importance of training in the basic principles of classical and modern physics and in mathematics is increasingly evident in the field of geophysics.[1] This need is being recognized in the relatively new branch of geophysics known as *oil well logging*. Well logging as practiced today may include acoustic, chemical, electrical, gravitational, magnetic, mechanical, optical, radioactive, seismic, and thermal methods.[2] It is highly important that the engineer or physicist doing oil well logging be thoroughly grounded in the physical principles of these methods. It is equally important for the physics or engineering student to recognize that here is another expanding field offering many opportunities for the application of ingenuity in research and development.

Oil well logging is done for a variety of purposes—tracing of migrations of fluids between zones, determining points of gas or water entry into a well, locating holes in casing, locating casing failures, testing of gas injection wells, identifying formations, locating the position of cement behind casings, structural studies and correlation between wells, locating gas-oil contacts, and, most important of all, locating the oil and thus establishing the best point of production.

An oil well log is a graphical record of some phenomenon such as temperature, electrical potential, electrical resistivity, or radioactivity as a function of depth. The logging instrument is contained within a tubular housing six or eight feet long and two or three inches in diameter (see Figs. 1 and 2). One method of obtaining a log is to lower the instrument into a well to some desired maximum depth by means of a cable wound on a large drum. It is then pulled up at a constant rate, synchronized with the recording instrument, and the phenomenon recorded as a function of depth. Similarly a record may be obtained while the instrument is descending. The instrument is sometimes self-recording and the supporting cable is simply a large piano wire of high tensile strength. In other systems the recorder is at the surface, the information from the detecting element to the recorder traveling by way of specially constructed electric cables.

Logs may be run in open holes, cased holes, flowing wells, or shut-in wells. Each type of log has its particular advantages. In general, radioactivity logs are better than electrical or temperature logs for structural studies in cased holes. Electrical logs are usually run in open holes before the casing is set. They are used principally in the logging of formations. Temperature logs have many special applications such as locating the position of fresh cement while it is setting, locating gas or water entry in open or cased holes, etc. Certain valuable information may be gained by first running a temperature log in a flowing well and then running a second log on the same well after it is shut in.

As early as 1918 depth-temperature graphs had been made by plotting a number of temperature readings as a function of their depths. Electrical logs were made as early as 1926.[3] But the practice of systematically making a continuous record of temperature or electrical potential as a function of depth and the idea of interpreting anomalous temperature and potential gradients is more recent. In many cases the *gradient* of a curve is more important than the absolute value of the reading at a particular depth. This gradient helps to locate changes in temperature, potential, radioactivity, etc., which in turn help to determine the location of formations, fluid intrusions, casing failures, and many other conditions in wells. In many instances it has been possible to reactivate abandoned wells through studies of logs. In other cases new pools have been discovered that had not previously been known to exist.

Temperature Well Logging

Lord Kelvin made what was probably the first measurement of a physical property in a well

[1] Clewell, "Education of physicists for petroleum exploration and production," *Am. J. Physics* 16, 483–5 (1948).
[2] Guyod, "Electrical well logging," *The Oil Weekly* 114, No. 10, 38 (Aug. 7, 1944).
[3] See reference 2, p. 7.

when, in 1869, he used a thermocouple to take temperature readings in a well 350 feet deep.[4] The core of the earth is thought to be extremely hot; at least the evidence seems to support this idea. There is thus a continuous flow of heat H by conduction from the earth's core toward the surface. The time rate of flow is, by Fourier's law of heat flow,

$$dH/dt = kA\,dT/dx, \tag{1}$$

where k is the heat conductivity of the material through which the heat is flowing, A is the cross-sectional area normal to the flow, and dT/dx is the temperature gradient. The amount of heat flowing per unit of time through successive layers of the same cross-sectional area is a constant. We may thus see that as heat flows through successive layers toward the earth's surface the temperature gradient is inversely proportional to the heat conductivity of a layer. That is, rearranging Eq. (1)

$$dT/dx = k^{-1}(dH/A\,dt),$$

where the quantity in parentheses is a constant for all layers (not necessarily the same at all places on the earth's surface, but fairly constant for a given locality). A layer of high conductivity should show a low temperature gradient and vice versa. Even though theory shows that it should be possible to distinguish between different layers due to their differences in heat conductivity, temperature logging has not been extensively used in the logging of formations. Temperature logging is more useful in such problems as location of fluid migrations, location of oil-gas contacts in open holes, location of holes in casing, and location of top of cement behind casings that have recently been cemented in. When a well is cemented in, for the purpose of isolating zones, it is of importance to know how far the cement extends above the point where it was squeezed in. Cement generates considerable heat in setting and hardening, and a definite temperature anomaly (a deviation from the normal geothermal gradient) will exist on a temperature log at the top of the cement, provided that the log is run within a few hours after the "squeeze job" has been performed. Figure 3(a) illustrates how a typical temperature log should appear for a location of cement level. Figure 3(b) shows how a typical temperature log should appear for a well having a point of gas entry. The entry of gas into a bore hole with its resultant adiabatic expansion produces a drop in temperature. This decrease in temperature occurs just above the point of entry where the gas tends to rise to the surface. Notice that depth is plotted vertically and temperature horizontally on these logs. The slope of the graph at any point is therefore dx/dT or the reciprocal of the gradient. Hence, the term ***reciprocal gradient*** is frequently used in speaking of the slope of a log. It is common practice, however, among logging engineers to speak of the reciprocal gradient simply as the ***gradient***, the reciprocal condition being understood.

The thermal element in the logging instrument is usually at the lower end of the tube. The balance of the tube may be filled with auxiliary apparatus such as automatic recording equipment, batteries, etc. A method used by the Dale Company of Los Angeles employs a bimetal coil to which a small vane is attached. Variations in

[4] Guyod, "Temperature well logging," *The Oil Weekly* **123**, No. 8, 35 (1946).

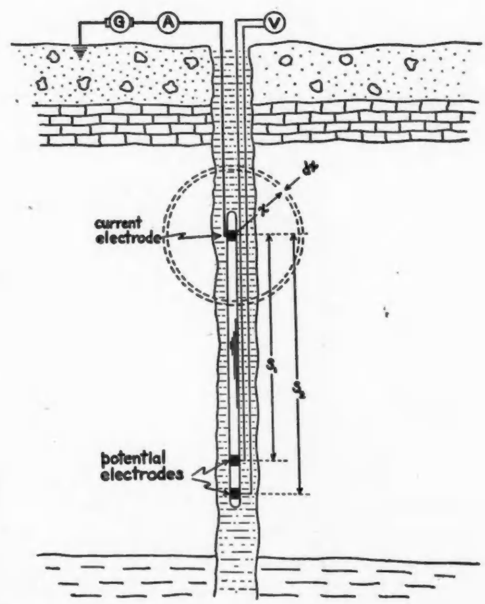

FIG. 1. Schematic arrangement of the standard three electrode method of electrical well logging.

temperature result in movement of the vane which in turn varies the position of a spot of light shining on a continuously moving 16-mm movie film. The motion of the film is synchronized with the movement of the logging instrument. After the log is run the film is removed from the instrument, developed, and then may be enlarged to 40× by projection onto a screen. In self-recording instruments of this type large, expensive cables need not be used. Instead a piano wire supports the instrument and the entire ten or twelve thousand feet may be rolled up on a comparatively small drum.

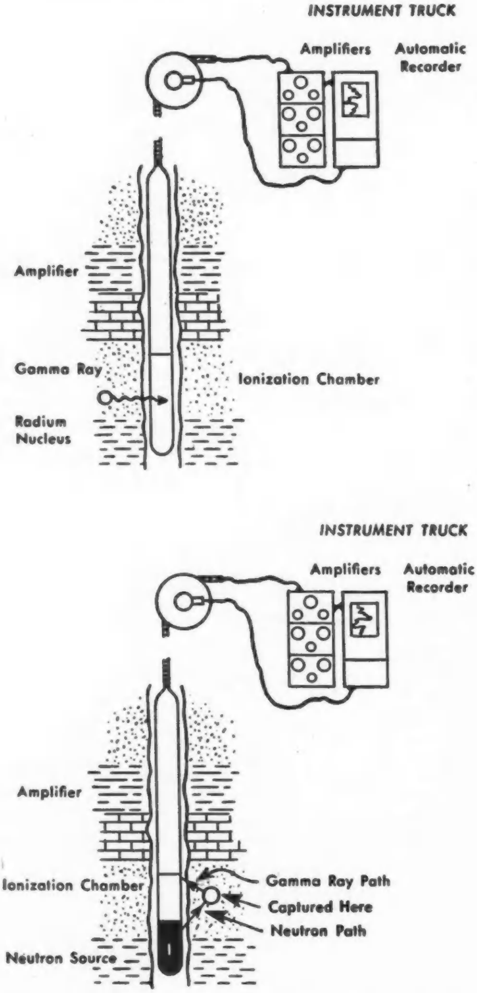

FIG. 2. Gamma-ray and neutron well logging systems (courtesy of Lane-Wells Company).

Another method of temperature measurement is to use a thermocouple as the detecting element. The thermocouple may be connected in one arm of a Wheatstone bridge. Temperature variations will then result in upsetting the balance of the bridge and this information may be recorded in the instrument or amplified and transmitted by way of electric cables to the surface. Whatever type of thermometer is used it should be sensitive enough to read to at least a fifth of a degree and have a very low thermal lag.[5]

Electrical Logging

There are two general methods of electrical well logging. The first makes use of the measurement of *natural potentials* existing beneath the earth's surface. These potentials may amount to as much as 100 millivolts. There are several causes recognized as the source of these natural potentials, the most significant one being the electrochemical potential created by the difference in saline concentration between the fluid present in the formation and the mud used in drilling operations. A typical electrical log of natural potential is illustrated in Fig. 4. These natural potentials make it possible to obtain fairly satisfactory logs with relatively simple equipment. It is standard practice today to run electric logs on almost all wells during drilling operations.

The second general method consists of measuring the *resistivities* of formations by means of artificially introducing electric currents into the well. Logs made in this manner in open holes are particularly useful in identifying formations. The elementary theory of this method may be understood by reference to Fig. 5, showing electric current supplied by the generator G through an insulated wire W flowing radially outward in three dimensions from a spherical electrode E. The electrode is thought of as being a metal sphere imbedded in a homogeneous, isotropic layer of considerable dimensions and at a distance of several hundred feet below the

[5] C. R. Dale, "Thermal logging of producing wells," paper presented to Pacific Coast District of Am. Petroleum Institute, March 10, 1942.

surface. The resistance of a spherical shell of infinitesimal thickness dx surrounding this electrode will be

$$dR = \rho dx/A, \tag{2}$$

where A is the area of the shell and ρ the resistivity of the material composing the shell. The area may be written as $4\pi x^2$ and the equation becomes

$$dR = \rho dx/4\pi x^2. \tag{2'}$$

If r is taken as the radius of the electrode, the resistance presented by the formation out to a distance s will be

$$R = (\rho/4\pi)\int_{x=r}^{x=s} dx/x^2,$$

and

$$R = \rho/4\pi(1/r - 1/s). \tag{3}$$

Equation (3) may be solved for ρ and hence,

$$\rho = 4\pi R(rs/[s-r]).$$

If I is the current through, and V the potential drop across the resistance R, then by Ohm's law $R = V/I$ and

$$\rho = 4\pi V(rs/[s-r])/I. \tag{4}$$

Equation (4) shows that with a spherical electrode of a given radius r the ratio of the potential difference between the electrode and a point at a distance s from the electrode to the current supplied by the generator is proportional to the resistivity of the formation. That is to say

$$\rho = K(V/I), \tag{5}$$

where $K = 4\pi(rs/[s-r])$. The value of K may be calculated if the geometry of the system is regular. Otherwise it may be determined experimentally by measuring V/I in a medium of known resistivity. In any event it is a constant for a given arrangement.

The conditions described in the preceding simple theory cannot be exactly attained in an oil well. In the first place the potential electrode cannot be placed at P_1, but must be above or below the current electrode. This presents no particular problem, for the potential at any other point, say P_2 or P_3, on the equipotential surface surrounding the electrode will be the same. The real difficulty arises from the fact that measurements must be made in a bore hole, and if drilling operations are in progress the hole is filled with a drilling fluid, usually mud, of different resistivity than the formation. It might appear that the complexity of the situation in a bore hole would render it virtually impossible to obtain useful measurements of the resistivities of formations. However the difficulty is not such as to prevent the acquisition of useful data. The resistivity actually measured is an apparent resistivity which is a weighted average of the resistivities of the materials surrounding the electrode, including the drilling fluid in the bore hole. The drilling fluid is so close to the current electrode as to exert a greater influence than the surrounding layers. The measurement of $K(V/I)$, then, is actually a measurement of the resistivity of the drilling fluid more or less influenced by the presence of the formations. In spite of this fact the viewpoint is taken that formations are being logged with the drilling fluid exerting its influence

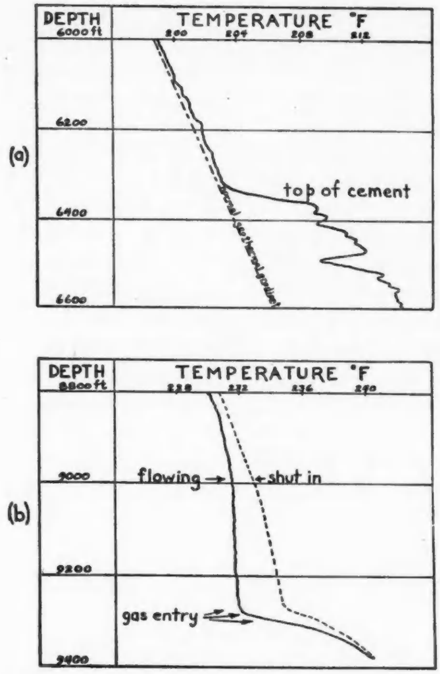

FIG. 3(a). Typical temperature log showing location of top of cement level. (b). Typical temperature log showing point of gas entry into a well (after C. R. Dale).

on the measurements.[6] The influence of the drilling fluid may be reduced by mounting the current electrode at the middle of an insulating mandrel several feet in length and having a diameter approximating that of the bore hole. This procedure is particularly effective where the drilling mud is highly conducting (i.e., saline). Another difficulty arises from the fact that the diameter of the bore hole is not necessarily constant. A resistivity log should therefore be compared with a caliper log of the hole diameters.

The method described in the preceding paragraphs is known as a *two-electrode* method. *Three-electrode* and *four-electrode* methods are frequently used. For example, a three-electrode system consists of a current electrode and two electrodes for measuring the difference of potential between two points past which the current is flowing as shown in Fig. 1. If these points are at distances s_1 and s_2 from the current electrode then it may be shown in a manner similar to the treatment of the two electrode method that

$$\rho = 4\pi V(s_1 s_2/[s_2 - s_1])/I. \tag{6}$$

The chief difference in the two methods is that in the two-electrode method the constant K of Eq. (5) depends upon the *shape* of one of the electrodes, while in the three-electrode system the value of K depends upon the *spacing* of the electrodes and is practically independent of their shapes so long as their size is small compared to the distance between them. In a sense the three-electrode system is a two-electrode system using a spherical current electrode of radius s_1. One advantage of the three-electrode method over the two-electrode method lies in the fact that the space over which the resistivity is measured is localized to that between the potential electrodes rather than including all the space between the current and potential electrodes. This results in better definition of layers. The three-electrode method presents similar problems of bore hole diameter and drilling fluid influence as with the two-electrode method, and the resistivities measured are apparent resistivities as before, but the response is considered to be more satisfactory. The apparent resistivities have a definite relationship to the true resistivities of the formations, although this relationship is not a direct proportionality.

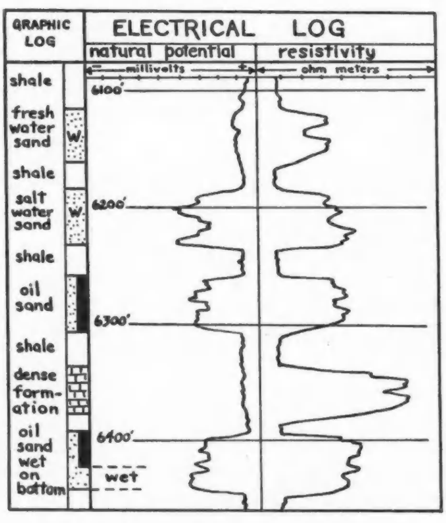

FIG. 4. Typical electrical log compared with a graphic log of the formations (after Lane-Wells Company).

[6] Guyod, "Electrical well logging, the single-point resistance method," *The Oil Weekly* 114, 44 (Aug. 21, 1944).

Other methods such as the *inverted three-electrode* and the *four-electrode* methods are used. Each has its particular advantages. The inverted three-electrode method, for example, is best where it is desired to log immediately below the bottom of a casing for evident reasons. The four-electrode method is a combination of the standard and inverted three-electrode methods. Figure 4 shows a typical electrical log comparing natural potential with resistivity.

Some Analogies

It is of educational value and sometimes of interest to the student to point out the close analogy between the flow of heat or electrical energy and the flow of a liquid or gas through a permeable medium. If we combine Eq. (2) with Ohm's law and let $I = dQ/dt$, then we may write for the time rate of flow of a quantity Q of electric charge

$$dQ/dt = (1/\rho)A\,dV/dx. \tag{7}$$

This is highly analogous to Eq. (1) if we let charge Q correspond to heat H, the electrical conductivity $1/\rho$ correspond to heat conductivity k, and the potential gradient dV/dx correspond to temperature gradient dT/dx. The ab-

stract nature of the flow of electricity and of heat may be clarified in the mind of a student by extending this analogy to the flow of a liquid of viscosity coefficient μ through a porous medium of permeability u. According to D'Arcy's law the volume dv flowing through a cross sectional area A in time dt is

$$dv/dt=(u/\mu)A\,dp/dx \qquad (8)$$

where u/μ may be called the ***hydraulic conductivity*** and is analogous to k and $1/\rho$, while dp/dx, the pressure gradient, is analogous to potential gradient and temperature gradient.

These analogies also have a practical engineering value in the field of oil well logging. For example, studies may be made of the flow of liquids through layers of media of different shapes and porosities by using electrical scale models. It is easier to make a measurement of potential with a voltmeter than to use a pressure gauge. Likewise it is convenient to use electrical models to study geophysical problems in the flow of heat. It is much easier to insulate an electric circuit than to provide heat insulation.

Radioactivity Well Logging

One of the most recent accomplishments in the well logging field is the use of radioactive phenomena in the logging of formations. This type of logging has a distinct advantage over electrical well logging in that more satisfactory logs are obtained in cased holes. Gamma-rays and neutrons may penetrate several thicknesses of casing. The Lane-Wells Company uses two different methods in radioactivity logging, the ***gamma-ray log*** and the ***neutron log***.[7] These methods may be understood by referring to Fig. 2. The gamma-ray logging instrument records the natural radioactivity of formations. This radioactivity is detected in an ionization chamber, preamplified, and then transmitted by way of specially constructed cables to an amplifier and automatic recorder in an instrument truck at the surface. The neutron instrument carries a source of neutrons at the bottom of the instrument. The neutrons may be produced by bombarding beryllium with alpha-particles from a radio-

[7] Lane-Wells Co., *Radioactivity Well Logging*, Bulletin RA-47-B (1948).

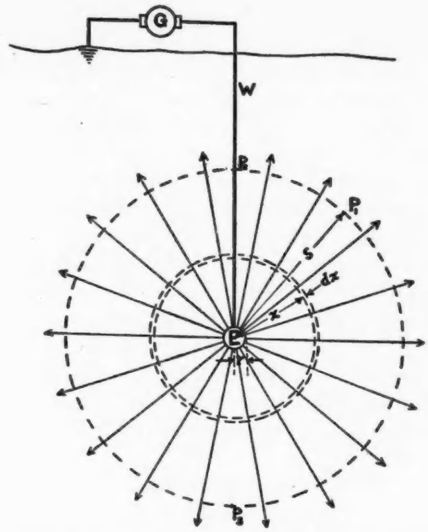

FIG. 5. Elementary theory of electrical well logging by the single point resistance method.

active element.[8] The beryllium is usually intimately mixed with the radioactive element, thereby providing a good target for the alpha-particles. The neutron source is shielded from the ionization chamber so that the neutrons do not enter it directly. Instead, the neutrons penetrate the casing and set up secondary gamma-radiations in the surrounding formations. The intensity of this secondary gamma-radiation is then measured in the same manner as with the gamma-ray instrument. Both types of logs are measurements of gamma-ray activity. The gamma-ray log is a measurement of the natural radioactivity of formations, while the neutron log is a measurement of the secondary gamma-radiation given off when formations are bombarded by neutrons.

The interpretation of the gamma-ray log shows that formations fall into three definite groups, formations of low activity, formations of high activity, and formations of abnormally high activity.[9] In the low activity group are sandstone, limestone, dolomite, salt, anhydrite, and cap rock. Shale, clay, and silt are the most common

[8] Fearon, "Radioactivity well logging, neutron bombardment of formations," *The Oil Weekly* **118**, 38 (June 11, 1945).

[9] Lane-Wells, Bulletin RA-47-B (1948), p. 13.

formations having a high order of radioactivity. Black or organic marine shale, granite, bentonite, and volcanic ash have abnormally high natural radioactivity. In general, igneous rocks fall into the third group. A typical gamma-ray log is shown in Fig. 6.

The neutron log shown in Fig. 6 is primarily a measurement of the amount of fluid in adjacent formations surrounding the instrument. The amount of secondary gamma-ray activity excited in formations by the neutron source overshadows the natural radioactivity. However, when neutrons strike a hydrogen-bearing formation such as water or oil, the secondary gamma-ray activity falls off markedly. These decreases in activity help identify fluid-bearing zones. Since

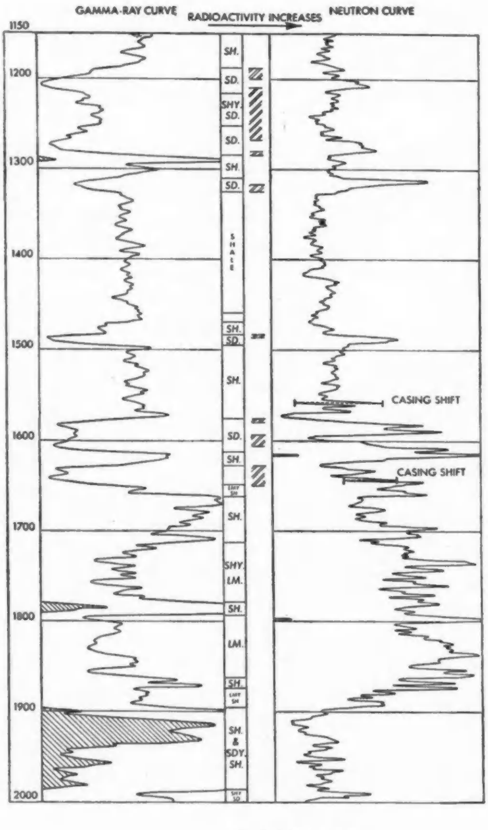

FIG. 6. Typical gamma-ray-neutron log (courtesy of Lane-Wells Company).

the hydrogen nucleus has approximately the same mass as the neutron, hydrogen is highly effective in reducing neutron velocity. Hence the fast neutrons are slowed down and are not as frequently captured with the resulting release of gamma-radiation as is the case with igneous rocks, sandstone, limestone, etc. The first neutron logs were made in 1941.[10]

The interpretation of radioactivity logs is still in a state of flux. Meaningless variations are quite common, and many anomalies exist which have not yet been explained to the complete satisfaction of petroleum engineers. Barcklow raises the question as to whether neutron logs are actually a measure of relative porosity rather than fluid content. He points out that in some cases relatively high intensities may occur opposite limestone sections showing oil staining.[11] Oil well logging is a wide open field for the physicist interested in research, investigation, and development.

Radioactivity well logging has made use of the idea of detecting radioactive tracers. If carnotite is mixed with the cement before squeeze cementing a well, the distribution of the cement behind the casing may be accurately determined with the gamma-ray instrument. Two ounces of carnotite to a sack of cement is sufficient to give a satisfactory indication on the measuring instrument.[12] A comparison between an initial run of the logging instrument and a run after the radioactivated cement has been squeezed in shows a clear indication of the top of the cement. This idea is illustrated in the gamma-ray curve of Fig. 7. Geiger-Müller counters were used in some of the early experiments with gamma-ray well logging, but the detecting device in most general use today is the oldest method of gamma-ray measurement, the ionization chamber.[13] Currents of the order of 10^{-13} ampere are measured by means of amplifiers and recorded at the surface. Radioactive tracers are also used in obtaining "relative permeability profiles" in limestone producing horizons for selective com-

[10] Pointecorve, "Neutron well logging," *Oil and Gas J.* **40**, 32 (Sept. 11, 1941).
[11] Barcklow, "Better interpretation of neutron curves," *The Oil Weekly* **118**, 53 (Aug. 20, 1945).
[12] Teplitz and Hassebroek, "Carnotite in squeeze cementing," *The Oil Weekly* **119**, No. 9, 41 (1945).
[13] Fearon, "Radioactivity well logging, gamma-ray measurements," *The Oil Weekly* **118**, 33 (June 4, 1945).

pletion of wells to inject gas or water into the producing horizons for secondary recovery operations. A bromide of radium may be converted to a salt or soap solution and mixed with the fluid for this purpose.[14]

Correlation between Logs

Graphical correlation between logs has produced some very important results. Correlations may be made between different types of logs (e.g., natural potential, resistivity, caliper, radioactivity, etc.) in the same well or between the same type of log in different wells in the same area. The former enables more certain conclusions to be drawn in interpreting logs, while the latter type of correlation has thrown much light on structural problems. By means of correlation of logs of old wells in the same area it has been possible to drill and economically produce new wells.[15] Figure 7 shows a typical electrical log, a gamma-ray log, and a temperature log of the same well near the bottom of the casing. The electrical log was run soon after the hole was originally drilled. Radioactivity logs made before and after squeezing in radioactivated cement are shown in solid and broken lines, respectively. The temperature log was run after squeezing in the cement.

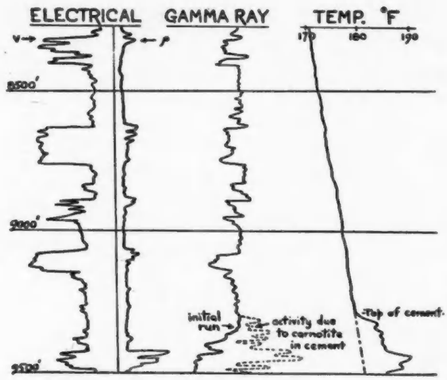

FIG. 7. Typical correlation between electrical, gamma-ray, and temperature logs (after Teplitz and Hassebroek).

Conclusions

Oil well logging is typical of the many new horizons of opportunity for physicists that have come into being in recent years, some of them directly, others indirectly, as a result of the war. It would be impossible to cover in a few pages all the different methods used in this field today. Fluid velocity meters, optical systems, magnetic devices, calipers, acoustic devices, electronic amplifiers, oscillators, etc., are all used in some manner or other. New developments are being made every day in well logging, and it is certainly a promising field for the prospective geophysicist. The educational requirements, however, should be centered around a strong fundamental training in the sciences providing a background to geophysics. The practical side of the problem can be learned in a relatively short time by the man with a good background in mathematics, chemistry, geology, electronics, classical physics, and modern physics.

The writer wishes to express his appreciation to C. R. Dale and Jack Dale for many illuminating discussions on this subject, and to the Lane-Wells Company and the Halliburton Oil Well Cementing Company for furnishing various pamphlets, magazines, and articles on oil well logging.

[14] Jackson and Campbell, "Some practical aspects of radioactivity well logging," *Am. Inst. of Mining and Met. Engrs., Tech. Pub. No. 1923*, Sept. 1945.

[15] John L. P. Campbell, "Radioactivity logs provide vital data for West Texas-New Mexico operations," *Tomorrow's Tools Today* (Lane-Wells Co.), Vol. XI, 3rd Qu., 1945.

Science increases our power in proportion as it lowers our pride.—CLAUDE BERNARD, 1813–78.

Observations of a "Reactionary" Physics Teacher

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WHEN I began teaching in 1902, physics was a good deal of a mystery to the general public. My friends insisted that I must be some kind of a chemist and Theodore Lyman of Harvard once told me that a young girl, hearing he was a physicist, asked: "Is it anything to do with a soda-water fountain?" What a change today! Physics has come into its own and is everywhere regarded as an essential part of the curriculum in both school and college. As we are living in a scientific age, physics is just as much of a cultural subject as the older humanities. Not to know something about the basic principles of mechanics, electricity, and, at least, to have a smattering of atomic structure stamps the modern man as only half-educated, just as ignorance of Latin and Greek indicated relative illiteracy to our forefathers. The question is not: *should* we teach our young people physics, but *how* should we teach them, especially those who have no pronounced scientific bent? To them physics is, and always will be, the most difficult subject in the curriculum. If it is made too easy, it is not physics, because from the very nature of the subject it demands the exercise of reason, and far too many people dislike using this highest of their mental faculties. So, what can be done about giving such pupils some understanding of the fundamentals of physics and their applications in our daily life?

The first and most necessary step in the preparation for the study of physics is *teaching the student to read.* An appallingly large number of college freshmen are quite incapable of getting information from the printed word. Thirty-five percent of secondary school boys, I am told, have great difficulty in recognizing familiar words seen in print, while an even larger number have such a pitifully small vocabulary that they are unable to use a textbook unless it is written in words practically of one syllable. This lack of ability is due partly to the so-called "progressive" method used in the early grades where children no longer learn the alphabet nor how to spell; and partly, I believe, to the insidious effect of the radio and the movies where one only listens and looks at pictures. The circulation of books, except picture books, for young people is said to be steadily declining in our libraries, and it is even proposed that librarians should show films to attract the young and so give a spurious impression of fulfilling the library's real purpose of education through books.

The fact that most students today are either unable or unwilling to read a textbook dawned upon me with startling suddenness when after a year of retirement I was recalled, during the war, for three more years of teaching a Navy V-12 unit at Trinity College. The boys in the unit came from all over the country mostly from public schools, and had not been picked with much reference to their previous record. Some, for instance, had never had any algebra. However, that deficiency was not as serious as their difficulty with reading. One day, shortly after I took hold of a large lecture class, I was practicing my usual method of going through one of the more difficult derivations on the blackboard, to make sure that every step was clear. Then I noticed that practically every one was busily taking notes. "Why are you doing that?" I asked. "It's all in the book." This statement was greeted with roars of laughter, and afterwards any remark of mine suggesting that the class consult the textbook became a standing joke. Their attitude was that it was cruel to expect them to dig out anything for themselves, and that it was my business to inject the scientific serum into their reluctant brains. One boy voiced this attitude when he told me: "The physics teacher I had in school was so poor that I actually had to get the stuff, myself, out of the book." Now the sad feature about this attitude is that there is not much we can do about it, so long as the baneful influence of progressive methods, cutting down on home work, elimination of examinations, and studying to the sound of the radio dominate the schooling of our young. The Russians, after copying our progressive schools, found their methods did not yield satisfactory

results, and for once the Soviet government was *reactionary* and went back to the good old ways. So we are the radicals in education and the Russians are the conservatives!

Next to being able to read intelligently, the student of elementary physics should have a reasonable facility with arithmetic, algebra as far as quadratics, about a dozen propositions in geometry, and enough trigonometry to solve right-angled triangles. That sounds like a modest requirement, but we all know how many students fall short of meeting it. Many are unable even to add numerical fractions or distinguish between a sine and a cosine, while they are terrified by any but the simplest algebraic expressions. So we have to take it for granted that many pupils are deficient not only in reading English words, but also in their ability to handle very elementary mathematics.

In view of the serious handicaps that must be overcome in teaching elementary physics, we are forced to adopt much of the technique of the tutoring school. We have to lecture more than sound pedagogy would call for, and we must take it for granted that books will be used only as a last resort by the majority of the class. But still, much can be accomplished, as I found with my V-12 classes. Even those, for the most part, poorly prepared boys could be made to learn definitions and the derivation of certain basic relations like $s=\frac{1}{2}at^2$, or $W=\frac{1}{2}mv^2$ and so forth. Of course the students did not like it and argued: "What is the use of just memorizing these proofs for an examination, and then forgetting them?" My answer was always: "If you don't do that, you will never look at the proof, and it is better to have learned it once—even by rote—than never to have read it at all." I know this is an unpopular attitude with many of my colleagues who require very few proofs of their pupils, if any. But this seems to me a serious mistake, and I recommend listing about twenty-five important relations for whose derivation the class should be responsible on the term examination, even though they will be brutally memorized by the majority.

You will see that I do not wholly despise committing some things to memory. Of course, we know that the ability to reason is the highest mental faculty we possess. Still the memory is a very valuable asset. It corresponds to muscular strength of the body as compared to the more subtle functions of the nerves. We need both in any physical activity such as playing a game of tennis, just as we need both reason and memory in the study of an exact science like physics. The modern tendency to frown on what is called "parrotlike memorizing" has been carried to an absurd extreme especially unfortunate in the pupil's earlier years, when he memorizes with enviable ease, and remembers what he thus acquired to a ripe old age. Some things that have no rational significance, like the alphabet, must obviously be memorized. Even the multiplication table is only a statement of facts that we all find convenient to learn by rote, and not to do so is just as silly as the prejudice some textbook writers have against using the Greek alphabet. They seem to argue that an eighteen year old boy is too young for such an exertion and that English letters are good enough for any red-blooded American. Actually any young person of average mentality can get the whole thing by heart in two hours if he has to, though he probably would consider the assignment unconstitutional as a cruel and unusual punishment. But the punishment is amply justified as soon as physics passes the kindergarten stage and when many symbols are needed to avoid confusion between the different quantities they represent. There are also certain algebraic statements of familiar laws that might as well be memorized. When you have to calculate kinetic energy you think at once of $\frac{1}{2}mv^2$ and do not want to take the time, either to prove it, or look it up in some table of physical relations. We all do it that way, so why discourage our pupils from using that heaven-sent gift, the memory?

After my defense of the use of memory, as well as reason, many of you will probably consider me hopelessly reactionary, so I may as well continue advocating certain old-fashioned methods as being superior to those of today. One of these methods is the oral recitation. It was still in vogue when I began teaching in 1902, but I confess to having used it less and less in later years, partly because of the large size of the classes, and partly because it bored both me and the students. However, I still think it produced results in some respects superior to what we get from written quizzes, and certainly superior to results obtained

from a straight lecture course, plus a final examination. A well-conducted recitation based on material assigned at the preceding session keeps the class on its toes. The questions used should be capable of concise and brief answers, such as the definition of a physical quantity or the statement of a law like Coulomb's law of electrostatic force. Then, if the answer is not immediately forthcoming, down goes a zero in the gradebook, and the question is passed on to the next boy. My own experience as a student is that I learned more from classes conducted partly as recitations than from the most polished lectures. Professor Van Amringe of Columbia taught calculus in the good old way, and you just *had* to know the lesson and be ready with an answer to an oral question such as: "What is the integral of dx/x" with no hemming-and-hawing, or be able to work a problem on the blackboard with no time lost in meditation. He did not so much *teach* us as make us *learn*. It was a drill-master method which would not go down today, but it worked!

Another of my old-fashioned views is that I do not believe in the inductive method of teaching in the elementary laboratory class. The pupils are too immature, and there is not enough time, anyway. I know there are advocates of turning a boy loose in the laboratory and letting him find out some law or principle for himself. He may get there in time, though he may break some costly apparatus in the process. Such a procedure means fewer experiments and results in less varied experience in the techniques of the laboratory. It is precisely the acquisition of technical skill and training in experimental methods that to my mind are the chief values of the elementary laboratory. Such a belief is rank heresy, for the laboratory is *supposed* to give the student a clearer understanding of the principles involved in an experiment. But this supposition is only partly true. Actually measuring something in the laboratory is not as illuminating an exercise as it is supposed to be. If, in an examination, the student is asked to explain the use of the Wheatstone bridge, for example, he is nearly as likely to flounder as if he had never worked with one. This result always surprised me, until I realized that the less thoughtful student overemphasizes results, and does not bother with the principles involved, especially if he only has to fill out spaces in a mimeographed report. To counteract this tendency the instructor should carefully explain the experiment before it is performed, in addition to whatever may have been said about the theory in the regular lectures, and if possible, he should question the student from time to time as he works, thus linking theory with practice.

In connection with laboratory instruction in electricity it is most desirable to make the student set up all the wiring from diagrams. In a laboratory I once visited, I noticed that in several experiments calling for rather complicated connections, the wires were all neatly nailed down to the table and the student had only to manipulate keys and switches. Such a procedure certainly saves time and trouble, but the especial benefit of acquiring technique is almost wholly sacrificed.

Finally, I believe in examinations—but not of the kind where you check the proper answer among a possible five, several of them usually preposterous. The only time I tried this type of examination with a class, the highest grade went to a second-rate student, who was smart and a good guesser. The best student of all, a boy who really understood the subject, came out with a considerably lower score. He was obviously confused by answers that would never have occurred to him in the older type of examination. For instance, if the question is "What does a galvanometer measure?" and, if the possible answers given are: *charge*, *current*, *resistance*, *potential*, *capacity*, the student might easily check *resistance* because, after all, he used a galvanometer with the Wheatstone bridge, whereas if he had been asked to tell how a moving coil galvanometer works, he would have answered correctly and his knowledge would be much better tested than when he had simply checked *current*. This modern type of examination saves the instructor lots of trouble and takes less time. It may even be necessary in connection with the mass-production methods of an overcrowded university, but it is certainly not the best way of testing achievement. The old-fashioned examination, however, if still used, should have fewer essay-questions than formerly, and should call for clear-cut answers such as definitions, statements of laws, basic proofs, and problems that

test the reasoning powers of the student, but are devoid of long and dreary arithmetical computations such as are involved in converting metric to British units or *vice versa*. In grading such an examination, I believe in giving credit for any glimmering of intelligence, even if the answer is partly wrong. A problem set up correctly and worked through intelligently may even have a wholly wrong answer and still deserve a grade of 90, if the error was due to one foolish numerical blunder. After all, we are teaching physics and not arithmetic! In general, I prefer a rather searching examination, graded sympathetically, to an easy paper graded with ruthless rigor. The former seems to me fairer to the student and it is much more illuminating to the instructor, provided he is willing and able to take the time and trouble to evaluate each answer with painstaking care and understanding.

The foregoing observations are the result of forty academic years of teaching not only elementary physics but many higher branches of the subject and at all undergraduate levels, as well as electrical and steam engineering, thermodynamics and physical chemistry. Such diversity of teaching, often necessary in a two- or three-man department, is not wholly desirable, but at any rate the instructor gains a very wide area on which to base his observations of the genus college student and of his needs. The foregoing recommendations and theories may sound reactionary—as indeed some of them are—but there are times when the world needs to recover certain values that have been lost through progress in a wrong direction. So, we teachers of physics need not be influenced by the charge of being reactionaries, provided the reaction is directed toward a sounder system of education. Older methods still have virtues to commend them in today's class rooms, crowded with poorly prepared students, the product of mass education, which has not taught them to read nor to manipulate the necessary mathematical apparatus of the elementary physics textbook.

Avoidable Dangers in the Rapid Development of General Education

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GENERAL education can become the most valuable single policy ever adopted by the liberal arts college, or it can be seriously, though unwittingly, sabotaged by its own participating teachers and administrators. Herein lies the greatest single danger to its permanent success. The reasons for this warning should certainly be reviewed.

The purpose of general education can be most simply stated in the words of Dr. Earl J. McGrath: "General education prepares the young for the common life of their time and kind." As we shall see, there are certain circumstances which deter the active teacher from facing this function squarely. To fulfill his purpose in general education he must break new ground for which his graduate study has only indirectly prepared him. One of my colleagues in charge of a general education course in the social sciences recently remarked that not only was the teaching of the course very difficult but also that his graduate training helped him directly not at all. This experience is probably common among those who take the purpose of general education seriously.

To make the discussion as specific as possible, consideration will be limited to the teacher of physics who is highly trained, having a Ph.D. degree. It is further assumed that he is interested not only in physics teaching in general education, but also in making a contribution to this field. At once he finds that he must make several adjustments. On the one hand, his mind revels in a subject which is gradually unraveling the physical nature of the universe, bringing out the laws which are expressible in mathematical form and which can be substantiated by quantitative experiments and computations. Physics is a subject which involves the beauty of mathematics and the attractive vitality of action. It is

a subject of marvelous intellectual architecture, one which ever entices its devotees by a challenge to creative imagination. It is in the direction having this perspective that our trained teacher must face if he expects his professional accomplishments to increase. But can he at the same time use his talents for the purpose of general education? It is to be observed that this objective requires him to face also in quite another direction, for only by so doing can he obtain his best answers to a number of questions. What does the study of physics offer to a nonscience student? What is its educational value in the common life among us? What parts of it can the nonscience student apply to life in the broad sense, and what can he use in his approach to his many problems as an individual member of society? Shall the teacher be controlled in his planning of the general educational course in physics by his own view of physics or by the view of the nonscientific citizen? Shall he take the standard textbook of college physics, which frankly is almost a skeleton, trim it down or skeletonize it yet more, and regard this skeletonized skeleton as that which physics has to offer? Physics glories in being a quantitative subject. Shall the teacher give the student enough computative experience to cause him to get a true view of this aspect? Does the teacher really know enough about actual life, or is he sheltered in a cloister where he cannot see one's need of physics in the common life about us?

What the physicist will do about his teaching aids in general education will depend somewhat upon the availability of such materials. The brief time during which physics has been active in general education has not yet produced needed physics textbooks and other materials. In the standard college physics course the guide up to now has been the requirements of professional training. The urgent need for giving this training has not left the physicist as free as the biologist has been. The physicist has been induced to believe that these traditional college texts are really physics and that nothing else can be. It is high time that the physicist evaluated these traditions and shakes himself free from them whenever he considers physics for quite another purpose. Clearly, if physics is to prepare the student for everyday living among us, the teacher should make a sharp break for complete freedom. The numerous technicalities in physics make this break necessary. In another subject the brief course in general education might be regarded as an introduction to that particular field, valuable to those who desire to continue in that study. But this sequence is not possible in physics if at the same time the purpose of general education is fulfilled.

The teacher cannot find freedom by an abbreviated survey of all the usual physics subjects, for such a course is a smaller serving from the same dish. To use another figure, the old man's suit cannot be trimmed down for the smaller son, for the material of the new suit must be essentially different. The effort of general education as a whole certainly should not be merely a broader appreciation and understanding covering more fields; rather it must involve, in each and every field, such as physics, an exchange of technicality for something more valuable. An important and more extended use should be made of less material. The student must have, with fewer facts and ideas, a significant intellectual experience. There are plenty, and indeed too many, things to accomplish in such a course. Physics in general education can contribute to the student's appreciation of any selected aspect, to his mental habits, to his manner of raising good questions and finding the answers with sufficient correctness, to his confidence in thinking independently, to his awareness and understanding of the physical world and the application thereto of what he has learned, to his reliance upon his general abilities everywhere applicable, and, in fact, to his self-respect as a thinking individual. These specific statements, though not complete, should not imply that all these objectives can be attained in any one course. The time is short and the teacher must select and emphasize only what may be possible to achieve. To do this he must certainly be more than a skillful and interesting instructor, more than a conveyor of information. For example, how can he encourage independent thinking if he has not had a serious struggle in this type of thinking himself? How can he be sensitive to the creative opportunities of daily life unless he has first become acquainted with creative activity as a scholar? Who can best prepare a student for

living—one who is full of static knowledge or one who has had his mind awakened to living at its best by using his knowledge productively or creatively? Clearly, the teacher must have been trained to be scholarly, he must have had a thoroughgoing creative experience such as is demanded for the Ph.D. degree. But he must resolutely face the needs of the nonscience student, assert his independence, and courageously select specifically what he will attempt to do in this short course. Even then, he must have great confidence and perseverance.

The danger is that he will lose heart. It is vastly easier for him to assume that here is only a very much shortened course of regular physics to teach and he will try, with apologies in his mind to *real* physics, to make the best of it. But even a moment's serious consideration should lead one to perceive that in a general education course there is a challenging opportunity, much greater than that in any advanced course which he is prepared to teach. He needs to have his thought constantly on the outside world, to keep his creative imagination highly active, and to make his approach ever flexible. It is his responsibility to give the highest possible educational value to this elementary course. What the students learn is, of course, important, but what they do with it is vastly more so. The writer does not refer to substitutions in formulas, to computations, and to the skillful use of units. Rather the students should emphasize drawing conclusions from the recognition and application of ideas in the world about us. Moreover, the teacher must carefully give factual information a subordinate position. Facts are and should be only the handmaiden of mind. Almost obviously, the danger here pointed out is one that the administrator of a college or of general education must fully understand. He must recognize the difficulty of this assignment, if the teacher's effort is to be amply rewarded.

These thoughts lead directly into teacher education as an aid to avoid the danger of ineffectiveness. If the teacher is to avoid falling into the error of duplication of high school teaching, of teaching the elements in an elementary way without using the processes of analyzing and organizing in addition to recalling, and if he is to give the course in physics the positive quality which it should have, some training in the graduate college must point in that direction. A little of the advanced factual knowledge usually required for the degree can be sacrificed for this training and without regret, for the Ph.D. physicist will naturally be acquiring information all the rest of his professional life. The graduate college is not a unique place in which to acquire information.

Will the graduate colleges make such a change? Twenty years or more ago they gave consideration to this subject, but nothing effective came of it, probably because nothing very practicable was presented. Today the need for high quality teachers in general education has made the subject more pressing than ever. The deans of the graduate colleges are once more taking up this problem seriously.

Since the title of this paper refers to "avoidable dangers," there should be at least some brief suggestions for the avoidance of the unprepared teacher. This makes necessary a more extended reference to the graduate training of college teachers. Two questions may be asked: what training should be added to the Ph.D. requirements as now generally constituted, and how should this training be provided?

A minimum of additional training can be described. The future college teacher should have a sensitiveness to social psychology and should be made aware of anything specific that the psychology of learning and of problem solving can offer. He should learn by experience in teaching elementary physics how to evaluate the cut-and-dry methods he may devise. The teaching, which can be in the standard elementary course, should be under careful supervision of a critical nature. The addition to graduate work need not be as large as may appear. For social psychology is chiefly the application of common sense, involving the recognition of the various aspects of the teacher's environment. Many of the important applications of the psychology of learning and thinking can be obtained through a few lectures by an interested scholarly psychologist in this field. This material can be increased by independent reading. The teaching internship must needs be under the critical eye of a member of the department concerned. Of these suggestions the last named is the most essential place

to *begin* the training of teachers in most departments.

In the preceding remarks the speaker is reflecting, though not quoting from, the opinion[1] of Dr. Claude E. Buxton of the Department of Psychology at Northwestern University. His opinion arises from experience in teacher training in elementary psychology. In it he assumes the following: one learns best to teach by teaching; though we are much in ignorance about what is good teaching, enough is known to make training justifiable; techniques cannot often be the same for any two teachers; learning by the trainee is most efficient if there is close supervision by a faculty member. Both graduate credit and pay is given for this work by the graduate student. It should be added that Dr. Buxton attaches more importance to social psychology in this training than to educational psychology, or any other study about teaching.

If this type of teacher training is essential in the effort to avoid the dangers accompanying a desirable general education, then it follows that the responsibility of any agency outside the department should be small in comparison with the responsibility assumed by the department itself. It is implied that, for the sake of the future of general education, the research scholars in a department should look with sympathy and interest upon the kind of training which has been mentioned. This is a critical condition for the success of the training of teachers. The sympathy and interest of the creative scholar will follow only if there is a definitely valuable education in these courses. It is obvious that the foregoing remarks emphasize not all that should be done, but only where and how to begin.

One of the most serious pitfalls of general education is one already incidentally mentioned. If the emphasis is put on breadth of knowledge, involving elementary consideration of a larger number of subjects, then it is easy for the teacher to adopt the goal of acquiring factual information. Students expect it. Examinations are easily prepared. The student memorizes and subsequently recalls what he has learned. Lectures can be interesting and easily organized. On the other hand, this paper has mentioned the very different goal of cultivating independent thinking. But, as has been seriously asked, how can one think independently without a knowledge of all the facts? Can any productive thinking be done with a very elementary knowledge of a subject? The answer is: certainly. Independent thinking is an act of the mind which does not need to be for the first time in the history of man, but only for the first time in the individual student's mind. Its historical value has nothing to do with the benefit to the student. But the educational disadvantage in permitting general education courses to lapse into the teaching of facts to be learned is a constant threat. Only the best quality of teaching can avoid this danger. Only the teacher who has developed confidence through experience and who is willing to have great patience can develop in the students valuable conscious mental habits.

Up to this point there has been emphasized the danger that the teacher of physics in general education will not break away from the guidance and purposes of the standard elementary texts for both engineering and liberal arts, that the teacher will not have suitable training while taking his Ph.D. degree, and that the teacher will too readily fall into the easy custom of permitting the knowledge of facts to dominate in the classroom.

One should also mention again the inherent danger in the circumstance that there are not available sufficient suitable materials for such courses. There is no need to dwell upon this difficulty, for if we have a sufficient demand the remedy will be produced automatically. The serious danger is rather in the lack of a positive aggressive attitude on the part of the physicists, for example.

But perhaps, to make this discussion less vague, there should be some comments concerning the possible nature of these texts and syllabi for the course in physics which are specifically for general education. What should they contain? Clearly they must include the material around which the teacher's selected objectives can be built. This material should be prepared for the purpose, and should not consist of selections in a much more comprehensive test. The omitted material would confuse and discourage. The teacher must break with tradition and organize

[1] Given in personal conference.

a course for the nonscience student who is to live in this world and have many problems common to us all. As examples, this is what has been done by Conant[2] in the appreciation of science study described in the critical study of a few historical problems, as at Colgate[3] and in the thought requiring question and answer course at Iowa.[4] In each of these there is opportunity for developing an appreciation of the function of physics in society, for application of principles in the world about us, for practice in independent and productive thinking, for the cultivation of conscious valuable habits and for the development of the student's respect for himself as a thinking individual. Obviously, there is too much opportunity to exploit to the full. The teacher must select wisely, I repeat, and specialize if he is to get a desirable result in a short course of not over four semester-hours. There are so many things the teacher can do that he is definitely embarrassed as to which way to turn. I may therefore be pardoned if there is now included as a very simple suggestion, three steps for the teacher desiring to establish or to recommend a course in physics for general education. First, he should acquire an understanding of the general purpose of the physics course (or the physics portion of a course) in general education and this is not as simple as it may appear. He must be willing to break with traditional courses in college physics. Second, he should seek out a suitable opening in the curriculum plans of the college for the work in physics. Third, with this opening in mind, he should try to envision what appropriate educational values are actually attainable in a reasonable degree in the short time available. Having taken these three steps thoughtfully, the teacher is ready to set his goals and plan his course. He will find some help and several general discussions in the book[3] edited by Dr. Earl J. McGrath, which contains articles regarding the practice in twenty-one colleges.

[2] James B. Conant, *On understanding of science* (Yale University Press, New Haven, Conn., 1947), p. 16ff.
[3] *Science in general education*, edited by Earl J. McGrath (Wm. C. Brown, Dubuque, Iowa, 1948), p. 39.
[4] See reference 3, p. 158.

This planning and its successful realization should be regarded as a challenge to the ambitious, well prepared, distinctly able physicist of high imagination and clearly defined purpose. But there is one prophecy which might safely be made. The educational success of physics in general education will never be determined by texts and lectures, but by *what the student does* with what is thereby provided. The necessary condition is that at all costs the active participation of the student must be secured. This cannot be overemphasized, but is easily neglected.

All of the foregoing discussion emphasizes the fact that general education is here and is too basic in purpose to be regarded as a passing phase. Notice that its advocates are not arguing for certain methods but for results, the need for which is unlikely to change. The methods are diverse and changeable, but the need is permanent. Therefore we must resolve the issue: How shall we develop a scholarly attitude toward, and a sufficiently high ability in the art of teaching for the success of physics in general education? We must at least be aware of the dangers and seek to avoid them.

This brings me to my next point, one that has often been stressed by other students of this subject. It is the extreme difficulty of giving away scientific secrets. I have never tried to do it, so I have no first-hand knowledge in this context. But I should imagine it would be rather like teaching. All of us have experienced the teaching process as receivers and some of us have also tried to serve on the transmitting end. Of course, if the secrecy goes so far as to include the mere fact of the existence of a project on a certain subject, such a secret can be given away without difficulty. But the amount of essential detail even with regard to principles and especially with regard to specific designs, that inheres in any modern scientific military device is fantastically great. To give away such secrets one would have to transfer vast quantities of drawings and documents. Even those are usually so unclear without explanation that the receiver would need to be given a special course of instruction in their meaning. Even this, to be really effective, requires the receiver to be a man of high scientific and technical training.—E. U. Condon (1948).

Did the Greeks Perform Experiments?

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A NUMBER of more or less popular books on the history of science have been published in recent years which stress the relations between the development of science and civilization. Such histories, however, seem frequently to have been written as if pledged to prove a parallelism between scientific advancement and cultural progress. Unfortunately, this attitude gives the impression that science and technical civilization of antiquity were on a very much lower level than those of our time. Although there has been a growing interest in Greek science, modern expositions written by scientists and scientifically trained writers are not always alive to the great importance of Greek thought in general and of Greek science in particular. As far as the latter is concerned, there is still much of the Whewellian spirit in the historical presentations of science of antiquity. In the words of a classical scholar, written a long time ago, but still appropriate,

> "The ancient philosophers have been charged by Dr. Whewell and others with wasting their fine intelligence in wrong methods of inquiry, and their progress in moral and political philosophy has been sometimes contrasted with their supposed failure in physical investigations."[1]

Greek Physics

The history of Greek physics especially is often presented superficially, with sweeping generalizations, and even somewhat disparagingly. The following passage from a valuable contribution by a meritorious author in the field of the history of science can be taken as typical—not for all enunciations in print, but for the opinion of the scientific work of the Greeks current among scientists and students of science:

> "[Observation and experiment were] . . . not to the taste of the Greeks. They were full of curiosity: they had a great artistic ability: but they always preferred the discussion of abstract principles to those practical measurements and weighings and prosaic descriptions of things that are the material of science. Simple experiments with tools and vessels and mechanical contrivances they felt to be slavish and degrading, so naturally they did not go far with physics and chemistry."[2]

Other authors are less sweeping on this point, but by the general reserve of their statements allow readers to form opinions even more hazy than those in the above passage. Historical introductions to modern physics textbooks, in consequence of their categorical statements in the same vein, often do more harm than good.

We still find that such discoveries of the Greeks as the spherical shape of the earth, the models of planetary motion, the laws of acoustics, or considerations about the properties of matter as were necessarily antecedent to their theory of elements and atoms and their physiological theories, are still presented as purely *speculative achievements*, and considered to be based respectively on 'aesthetical arguments,' 'number speculation' or the play of an 'unbounded imagination.' We can hardly believe that such knowledge is found so easily, that such truths can be discovered by inspiration rather than on the basis of accurate observation, collection of data, and planned experimentation. We cannot believe, as scientists, that we have here before us anticipations rather than generalizations.

Professor McCue has shown in a recent article in this journal[3] how much valuable material from the history of Greek physics can be included in the teaching of even elementary physics courses. But his paper is not free from a hesitancy to make a clear statement on the subject of Greek experimental physics, a hesitancy which is also apparent in his main source. In one paragraph he speaks of the Greeks' scientific studies "by

[1] B. Jowett, *Dialogues of Plato* (Hearst's International Library, New York, 1914), Vol. III, p. 388.

[2] F. Sherwood-Taylor, *Science past and present* (Heinemann, London-Toronto, 1945), p. 16.

[3] J. J. G. McCue, "Ancient science in the modern curriculum," *Am. J. Physics* **16**, 404 (1948). The following quotations are from pp. 404 and 405.

observation *or* experiment" (italics mine), and later on, in connection with their acoustical research he stresses, by using italics for a whole sentence, that "*This discovery must have been based on experiment*," as if pleading indirectly with a reader whom he suspects to be prejudiced on the subject.

It is hardly true to say, as I find stated elsewhere, that the Greeks have "failed to test their theories by experiment." Of course, they had not the apparative means of proving their very subtle and encompassing theories in all respects, any more than we are now able to prove directly the existence of the neutrino. But it cannot be maintained that their theories were without experimental basis and that they disregarded and despised the experimental method. As Burnet pointed out, the Greeks "were conscious of the need for verification. This they expressed by saying that every hypothesis must 'save the appearances'; in other words, it must do justice to all observed facts."[4]

From this point of view, the great variety of their theories seems to indicate that they were prepared to abandon theoretical patterns under the impact of new observations and experiments, performed in order to prove or disprove earlier concepts. It is, therefore, an oversimplification of the true situation when it is assumed, tacitly or expressly, that very little can be said with certainty about the work and the methods of antique physics, and that for this reason we have to be satisfied with superficial generalizations and unqualified statements. Our contention has the support of Ernst Mach who stated about 60 years ago, in revision of an earlier standpoint, that

> "The more we learn about the scientific literature of antiquity the more favorable becomes our opinion. . . . The opinion, until recently still in vogue, that the Greeks were especially neglectful of experiment cannot today be maintained to the same degree."[5]

[4] J. Burnet, *Greek philosophy*, Part I (Macmillan, London, 1914), p. 11.

[5] E. Mach, *The science of mechanics* (Open Court, Chicago, 1907), p. 509. I am here quoting from the German original.

A source book in the history of science, like that recently published by Cohen and Drabkin[6] should be influential in dispelling some of the common prejudicial opinions prevailing among physicists.

The situation from which we observe and judge Greek science today is somewhat similar to that which could arise a few thousand years hence if our modern literature were destroyed or lost to the same extent as is unfortunately the case with the scientific writings of the ancients. If some popular books only were preserved, perhaps Eddington's and Jeans' publications, and a few copies of the *Scientific Digest*, and similar materials which stress results and implications more than working methods, one would in later ages deduce that the twentieth century was distinguished in certain areas for its scientific foresight. Since some of our knowledge will undoubtedly be found wanting, the future scientific critic and historian will find our present achievements to have been a mixture of good and bad guesses.

Our present position in regard to Greek science is somewhat that of an archaeologist who has to reconstruct Greek edifices solely from fragments of Vitruvius' *De Architectura* or Pausanias' *The Description of Greece*, or of a classical scholar who would try to picture the Greek theatre without a knowledge of Aeschylus' and Sophocles' tragedies, and who has to base his arguments entirely on Aristotle's *Poetics*. Such an appreciation of Greek artistic culture and life could hardly amount to the same as that which is called forth by the study of the original work. The historian of science is less fortunate in this respect than his fellow-scholars in the humanistic fields. Besides, under the influence of Comte's and Whewell's philosophy of history, scientists themselves have taken only scant interest in the older history of science, misled by a belief that *inductive science* stems from Galileo and Bacon, and has been an exclusive achievement of the last three centuries.

Observation and Experiment

To maintain that classical scholars have ignored the study of Greek science does injustice

[6] M. R. Cohen and I. E. Drabkin, *A source book in Greek science* (McGraw-Hill Book Co., New York, 1948).

to their studies. Our information about the different periods of Greek cultural life is considerable, although it varies in degree for different periods; indeed, it is by the degree of available knowledge that we distinguish certain periods. It is to be noted that we often depend for information about events or personages on sources written 400 or 500 years later, that is, on second- and third-hand information. The study of Plato's and Aristotle's works is of great importance because we have here an extant original (or nearly original) literature. As far as the question of observation and experiment is concerned, we are on firm ground in maintaining that in the time of the Alexandrian school specialized scientific research was undertaken by experimenting physicists like Archimedes and Hero, and by observing astronomers like Aristarchus, Hipparchus, and Ptolemy. It is on this period that McCue's[3] article gives a valuable condensed collection of data and facts.

The criticism that the Greeks neglected experimental evidence is usually aimed at the earlier time of the Ionian philosophers, the atomists, and especially at the Athenian schools of Plato and Aristotle—the period in which the most important theoretical concepts were formed.

About the pre-Socratic scientists we know very little by direct information, but there is a store of evidence, of unequal value, available from the literature of the so-called *Doxographi.* However, it should be borne in mind that our understanding and apprehension of the pre-Socratics began during the last century only, exactly at a time when our own physical world-picture was based on a purely materialistic, atomistic (molecular) physics. The philosophies of the Ionians and of the Atomists and those of their opponents must have been intelligible at the time in terms of similar concepts of the various states of matter, and by reason of the study of their mechanical and thermal properties.

It is being recognized to an increasing extent that the age of Plato was not only a climax of moral and political studies, but of the most important scientific investigations and discoveries. More attention is being paid to the school of natural scientists, the neo-Pythagoreans, who in the time of Democritus and Plato developed science along unquestionably experimental lines.[7] Our evidence for scientific work of an experimental nature in Plato's age is mostly indirect, but we must not overlook the fact that this is only one out of many historical problems in need of clarification—the question of the organization of the Academy, the methods of instruction applied, the library connected with it—and we cannot expect to find *expressis verbis* a description of a scientific laboratory.[8]

We have, however, well-established knowledge concerning the Platonic age that occupation with a special science required a man's life, that division between physical and biological scientists prevailed, that medical schools had a high scientific standing, and that the literature of every science filled a library. We have also a number of utterances ascribed to Socrates and Plato directed against observational science. The ethical relativism of the Sophists, who were originally also teachers of the sciences, is supposed to have sprung from the scientific relativism which they developed in contrast to the more naïve natural philosophy of the Ionians and the materialistic schools. In the classical literature evidence from the field of Greek comedy is widely accepted, and rightly so, as the stage gives a distinct, although perhaps distorted, picture of a period. Having succor from this kind of argument, we find that in Aristophanes' comedy *The Clouds*, a play directed against Socrates, the philospher is derided as a 'sophist' and also as a sort of 'quantitative biologist,' who by rather elaborate means reportedly measures the length of a flea's leg. The play throws some light on the geographical and astronomical work of the time. We can consider this stage play as evidence for scientific experimental work, which apparently was incomprehensible to the great mass of the Athenian population, but known well enough to permit public misconceptions to be utilized by a mischievous playwright.

Aristotelian physics is often the special target

[7] Erich Frank, *Plato und die sogenannten Pythagoräer* (Halle, 1923). See also G. C. Field, *Plato and his contemporaries* (London, 1930), Chap. XIII, p. 175; F. M. Cornford, *Plato's cosmology, a commentary to the Timaeus* (Harcourt, Brace and Co., New York, 1937).

[8] H. Cherniss, *The riddle of the early academy* (University of California Press, Berkeley and Los Angeles, 1945).

of critical remarks about the Greek aversion to experimental research, specifically in connection with the (supposed) wrong observation that bodies fall with velocities according to their weights. But we could pose the question whether the basis of such a statement was not a more or less correct observation. Did they perhaps make experiments with falling bodies from very high cliffs or in chasms, and observe the difference in time of fall due to air resistance? We know that Galileo's experiments at the leaning tower of Pisa, if performed at all, would have been favorably influenced by the relatively small height of the tower. Or had the fall of bodies in viscous liquids been observed, and from the results of such experiments the inference drawn that in principle the same conditions exist in the fall through air? Be that as it may, Greek scientists, although unable to observe the fall of bodies in vacuum, must have known that in empty space all bodies would fall with equal speeds. The assumption of Democritus' cosmology that atoms fall with velocities according to their weights and so give rise to the formation of material bodies by impact of the faster with the slower atoms, was replaced by Epicurus' atomistic theory which included 'self-activity' of the atoms (i.e., molecular forces), in order to explain the interaction between the smallest particles. Lucretius reports on this development. We cannot persuade ourselves that the change of theory was due to speculation rather than to additional experience on the free fall of bodies after more experimenting had taken place.

The historian of science may have to re-evaluate existing material, and look for evidence not only in the narrow domain of extant scientific works but in the wider field of all classical written material. Even on the basis of our present knowledge the question should no longer be whether the Greeks performed experiments, but only why we have so little evidence of their experimental work. The remaining problem is whether the loss of written documents is the single cause of the paucity of direct information on the Greeks' experimental work.

The Professional Code

There is little doubt that much written material has been lost in the course of centuries, and that much has been deliberately destroyed, like the works of Democritus. But I suggest that the reason for the absence of evidence for the experimental work in particular may perhaps be found in the Greeks' neglect of recording scientific results, especially those of an experimental nature. In short, I suggest, we should admit that not so much have the methods of scientific inquiry changed, as have the *mores* of the scientist. This does not mean to say that the Greeks had experimental institutions as extensive as ours are today, or that their physics and technology overshadowed their civilization to the same degree as is the case in our own.

Much of their knowledge the Greeks considered as *mysteries*, i.e., as secret knowledge, to be passed on only to a selected few in sacred communities, scientific schools, and trade-guilds. A special scheme of *esoteric* teaching existed besides the popular *exoteric* teaching, not unlike the secrecy adopted in some modern physical activities, though for different reasons. (In connection with this I do not wish to imply that their secrecy explains the withering away of Greek science after a fruitful period of several centuries, or that history will necessarily repeat itself.) As a matter of fact, their higher studies were hidden under a mythical cloak. The fad adopted by the neo-Pythagoreans, an important school of natural scientists in Southern Italy, of antedating their latest discoveries, pretending that they were traditional knowledge which came to them from the 'master' Pythagoras (*Ipse dixit*) achieved the same concealment of their working methods, experimental and theoretical.

We have already observed that the lack of written evidence concerning the extended use of experimental methods led to the supposition that the Greeks had a *gentleman's* attitude towards manual laboratory work, that they shirked experimenting. So formulated, the assumption is more in the spirit of modern snobbishness than in agreement with the standards of conduct of Greek intellectual and artistic work. It can easily be argued that this explanation is quite untenable, because the Greeks could have used slaves for making their observations, and for building their scientific apparatus (which they very likely did), in order to satisfy their

admittedly high curiosity. It is, however, true that labor for the Greek was considered a calamity, and that a life of leisure was extolled as the only dignified life for the free man. But this applied not only to manual labor; it applied as well to the work of the poet, the sculptor, and the scientist.

It applied with one important qualification: for the creative artist (and the scientist, we will add), it was not undignified, by his own code, to do even menial work in connection with his task, because he considered such an undertaking to be enjoined by a higher, superpersonal power. Such work had its own dignity. The attitude is well expressed in the following lines:

> "To the Greeks the work of the artist falls just as much under the undignified conception of labor as any ignoble craft. But if the compelling force of the artistic impulse operates in him, then he *must* produce and submit himself to that need of labor."[9]

The artist's or scientist's own code did not require him to hide the manual part of the work, and certainly would never have made him forego the enjoyment of creating because of the social slight put upon him. But with such a restraining attitude with respect to artistic, intellectual, and what we would now term scientific work, he must have had at the same time a desire, if only for the sake of increased social prestige, to uphold before the uneducated and uninitiated the pretension that the results of his work were obtained by inspiration and vision rather than by labor: 'Minerva springing fully grown from the brow of Jove.'

Our modern philosophy of publication of methods and results makes it hard to understand the different attitude in ancient Greek times. It should be remembered, however, that the modern poet presents only his finished work, that the unveiling of a work of art may be a residue of the Greek classical attitude to make the work appear as a finished product, and that modern popular opinion still demands artistic and scientific work to be presented mainly as an effortless, graceful creation, as the world of the film particularly demonstrates.

In many instances the negative attitude of Greek scientists toward the publication of their working methods may have been influenced by thoughts of conforming with the standards of the nobly born and leisured class. But some of them may have been convinced that the artist or scientist is "a tool of manifestations of will infinitely greater than he is permitted to consider himself in the isolated shape of the individual."[9] Such a feeling of personal insignificance has been expressed many times by great men of science in all centuries. Probing nature with human tools and instruments (modest as they were compared with ours), appeared to them like a Promethean venture; the public discussion of these investigations must have seemed to them like *hybris*.

The dark and deep feeling of an earlier time may have gradually given way to conscious pretension or unconscious mannerism. The deductive method of representation, as used by Euclid, may have been used as much for these reasons as for reasons of logical construction. The scientist of the Platonic age reveals his results, at the best, only in dark allusions; the nobleman Archimedes of a later date gives results rather than detailed reports of proofs; after another century the commoner Hero has lost some of the earlier social inhibitions, and from him we already learn much about the working of his machines. But by this time the creative period of Greek science has already dawned.

[9] Friedrich Nietzsche, *The Greek state*. Complete works (Edinburgh and London, 1913), Vol. II, p. 5.

The way in which the persecution of Galileo has been remembered is a tribute to the quiet commencement of the most intimate change in outlook which the human race had yet encountered. Since a babe was born in a manger, it may be doubted whether so great a thing has happened with so little stir.—A. N. WHITEHEAD.

Reproductions of Prints, Drawings and Paintings of Interest in the History of Physics

44. *Vanity Fair* Caricatures of George Biddell Airy and Richard Anthony Proctor

E. C. WATSON
California Institute of Technology, Pasadena 4, California

ASTRONOMY is so closely allied with physics, and astronomers have so often been physicists as well, that no apology is necessary for including the caricatures of two eminent astronomers in this series of historial reproductions.

This is particularly true of one of the two astronomers whose *Vanity Fair* caricatures are here reproduced. GEORGE BIDDELL AIRY (1801–1892) did outstanding work in mathematics and physics as well as astronomy, particularly in his younger years. His work on the theory of light which in 1831 won him the Copley medal of the Royal Society and his researches on the mean density of the earth were especially noteworthy contributions to physics.

RICHARD ANTHONY PROCTOR (1837–1888) was a popularizer rather than a creator of astronomical science. His writings exerted a wide influence in familiarizing the public with the main facts of astronomy and he was well known as a popular lecturer not only in England but in America and Australia as well.

Sir George Biddell Airy, K.C.B.

"We have among us in the various departments of Science some truly great men whose names will live in their work to many future ages; and of these is Sir George Airy. Born in Northumberland four-and-seventy years ago with a splendid intellect but to no inheritance, he has made of himself, by an unremitting course of labour of the most trying kind, what he is—one of the glories of his country. Not without difficulty he succeeded at eighteen in entering Trinity College, Cambridge. He came out Senior Wrangler, was elected Lucasian Professor at twenty-five, and at once proceeded to deliver a most remarkable series of lectures on Experimental Philosophy, in which he fully developed for the first time the undulatory theory of Light. At twenty-seven he was elected Plumian Professor, and now he took charge of the Cambridge Observatory, and devoted himself with all his rare powers to astronomy. The best mathematician of his time, and with a natural turn besides for the more delicate forms of mechanics, he at once began to revolutionise all the astronomical calculations, and to perfect the observations by adapting to them every modern resource of the mechanical arts; and at thirty-four he was taken into official recognition by receiving in the post of Astronomer-Royal one of those few appointments which must even in these times be given

PLATE 1. Sir George Biddell Airy, K.C.B. [From *Vanity Fair*, November 13, 1875.]

solely for ability and aptitude. In this capacity he has served the State and the Science like the enthusiast that he is, nor could there be named a man who has done so much and such wearing work as he. He superintends the compilation of the Nautical Almanack, he is appealed to on all questions of boundary, he rates chronometers, and corrects compasses, and withal he finds time to organise expeditions, to start new theories in optics, and to contribute many papers to the public press. A sober, steady man, with an immense capacity for and delight in labour, his life has been spent where his work lies, on Greenwich Hill, and he is little known to Society; yet he is still young, he knows almost everything, and his accomplishments and simplicity render him the most charming of companions. It is remarkable proof of the estimation in which men of the highest worth are held in England, that at seventy Sir George was made a Companion and at two-and-seventy a Knight Commander of the Bath. Perhaps some day, when he has performed as great service to his country as Prince Leiningen and Sir John Pakington, he will be admitted with them to the honour of the Grand Cross, which will nevertheless make him no greater a man than he is."

PLATE 2. Mr. Richard Anthony Proctor, B.A., F.R.A.S. [From *Vanity Fair*, March 3, 1883.]

Mr. Richard Anthony Proctor, B.A., F.R.A.S.

"When Mr. Proctor went to Cambridge College over twenty years ago, he had no thought of becoming a prophet. He had learned several languages more or less efficiently, and he had excellent mathematical capabilities. After beating all the best men of his year, and proving that he could be Senior Wrangler if he chose, he grieved the academic heart by deciding not to choose. Rowing and riding seemed to him very much better than thermodynamics and curves as objects of human interest; so he rowed and rode instead of earning the affection of coaches. The moral proceedings of Messrs. Overend and Gurney startled him in the midst of his athletic career, and he saw that the universe contains many things besides outriggers and hacks. Having lost his fortune, he settled down to steady work, and, to beguile the world, he wrote a little treatise on cycloids which is the best thing of its kind ever done by an English mathematician. Eighty people in the world rushed upon this book and bought it; but, so far from being satisfied with this unprecedented appreciation of his years of labour, Mr. Proctor declared that he would have a larger audience. He therefore devoted himself to astronomy, and became the secretary of the Royal Astronomical Society—to the enormous dismay of the fogies. He has lectured in America and Australia; he has written innumerable essays of the brightest and clearest kind; he has sketched, modelled, gone into training, started a newspaper, and married an American lady. These feats caused the fogies to esteem him a heterodox. At present he is editor of *Knowledge*, and his journal seems to please many people. He is five-and-forty years of age, he has invented a new theory of the universe, and he is afraid of growing fat."

RECENT MEETINGS

Southeastern Section, American Physical Society

THE 15th annual meeting of the Southeastern Section of the American Physical Society was held at Clemson College, Clemson, South Carolina, on Friday and Saturday, April 15 and 16, 1949. Approximately 325 members and guests attended.

The program consisted of 67 ten-minute contributed papers and five addresses by invited speakers. The titles of the invited papers and abstracts of three of the contributed papers of interest to physics teachers are printed below. The others will be printed in the *Physical Review.*

Invited Papers

Rocketborne upper atmospheric experiments of the Air Materiel Command. MARCUS O'DAY, *Watson Laboratories.*

High speed rotors. J. W. BEAMS, *University of Virginia.*

New developments in infrared spectrometry. EARLE K. PLYLER, *National Bureau of Standards.*

Search for beta-proton coincidences associated with neutron decay. A. H. SNELL, *Oak Ridge National Laboratory.*

The Oak Ridge National Laboratory. A. M. WEINBERG, *Oak Ridge National Laboratory.*

Contributed Papers

1. Physics for humanities majors. N. GOLDOWSKI, *Black Mountain College.*—The purpose of this paper is to try to answer two fundamental questions raised by the introduction of physics into the liberal arts curriculum:

1. What is the reason for teaching physics to humanities majors?

2. What is the method of approach to the subject in this particular case?

These two questions are interdependent since the method of teaching is determined by the goal one wishes to achieve.

The reason for teaching is described as the necessity of developing in the student the capacity (1) for strict analysis and rigorous synthesis and (2) for correlating fundamental laws of physics to other branches of knowledge.

The process of instruction should consequently consist in an attempt to train the student in the methods of relating experimental data to the general laws and to point out the significance of these laws in the domain of the humanities.

The material should include all the fundamental ideas of physics which can be expressed in simple mathematical language even if the derivations of some of them have to be omitted because of the inadequacy of the mathematical background of the usual humanities student.

2. The use of polystyrene to improve electrostatic equipment. A. R. REED, *Clemson Agricultural College.*—This paper is a short discussion of problems encountered in electrostatic demonstration and improvements in apparatus by the use of polystyrene insulators.

Polystyrine has an electrical resistivity equal to or higher than that of newly cast sulfur and is free from the latter's undesirable properties. It makes a much better charging rod and electrophorus. Pith balls sometimes remain charged for an hour when hung from polystyrene insulators and electroscopes remain charged overnight. With such equipment a satisfactory demonstration is possible during the worst conditions of high summer humidity; this was seldom even attempted in the past.

The presentation will be accompanied by an exhibit and demonstration of several pieces of equipment including a gallon-size electroscope.

3. Demonstration of image formation by a convex lens. WALDEMAR NOLL, *Berea College.*—At least three real images formed by a convex lens can be computed using well-known equations of reflection and refraction at surfaces. This demonstration has been given at the University of Iowa Physics Colloquium.

At the business meeting, the following officers were elected for the next year: *Chairman,* J. H. HOWEY; *Vice Chairman,* W. G. POLLARD; *Secretary,* A. D. CALLIHAN; *Treasurer,* H. F. HENRY. F. G. Slack was elected to the Executive Committee for a four-year term. The next meeting will be held at the University of Chattanooga, Chattanooga, Tennessee.

ERIC RODGERS, *Retiring Secretary*

American Association of Physics Teachers

District of Columbia and Environs Section

The annual meeting of the District of Columbia and Environs Section of the American Association of Physics Teachers was held on April 2 at Goucher College, Baltimore, Maryland. Eighty-one members and guests were registered. The morning session consisted of contributed and invited papers. At noon the groups visited the statitron at the Johns Hopkins University. In the afternoon there was a panel discussion followed by several additional contributed papers.

Invited Papers

1. The statitron of the Physics Department at Johns Hopkins University. D. R. INGLIS, *Johns Hopkins University.*

2. The atomic clock. HAROLD LYONS, *National Bureau of Standards.*

Contributed Papers

1. Brightness and field of view of the telescope. CARL A. BECK, *Catholic University of America.*

2. Controlled atmosphere for aircraft engines. C. H. VOELKER, *Washington College.*

3. Experiments for a lecture on momentum. RICHARD T. COX, *Johns Hopkins University.*

4. Operational methods in mathematical physics. L. B. ROBINSON, *Howard University.*

5. Visualization of normal coordinates of coupled oscillators. D. R. INGLIS, *Johns Hopkins University.*

6. Use of nuclear plates in teaching. F. L. TALBOT, *Catholic University of America.*

7. A research project as motivating factor in high school physics. E. P. HEINRICH, *Sidwell Friends School.*

8. Gadgets used in simple demonstrations. R. L. FELDMAN, *Roosevelt High School.*

9. Confetti and Turbulence (Read by title). E. W. THOMSON, *U. S. Naval Academy.*

Panel Discussion

The relation of physics and the physical sciences to the general education student. OTTO KRAUSHAAR, President of Goucher College; B. D. VAN EVERA, Coordinator of Scientific Activities and Professor of Chemistry, *George Washington University.* Discussion Leaders: J. J. HINSON, *City College;* R. A. GOODWIN, *U. S. Naval Academy;* W. A. KILGORE, *Wilson Teachers College.*

At the annual business meeting, the following officers were elected for the coming year: *President and Representative on the National Executive Committee*, VOLA P. BARTON, *Goucher College; Secretary*, E. R. PINKSTON, *U. S. Naval Academy; Executive Committee*, G. W. KOEHL, *George Washington University;* VOLA P. BARTON, *Goucher College;* CLAIRE DRISCOLL, *Central High School, Washington, D. C.;* MARION KRAUSE, *Eastern High School, Baltimore, Maryland;* E. R. PINKSTON, *U. S. Naval Academy.*

E. R. PINKSTON, *Secretary*

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ANNOUNCEMENTS AND NEWS

Book Reviews

Principles of Physics III. Optics. Third Edition. FRANCIS W. SEARS. Pp. 369+xviii, Figs. 287, 15½×23½ cm. Addison-Wesley Press, Inc., Cambridge, Massachusetts, 1948. Price $4.50.

This book is the third in a series of texts written primarily for a two-year introductory course in physics given at the Massachusetts Institute of Technology. Although a familiarity with calculus is assumed, the treatment is not far above the level of most textbooks in college physics.

The field of optics is covered very well and the arguments are easy to follow. The illustrations are especially good. There are many perspective drawings which help to clarify the three-dimensional aspects of optical phenomena, particularly in regard to polarized light. There is a good assortment of problems, some of which are solved in the text. Answers are given to alternate problems. The terminology follows modern conventions. This is particularly noteworthy in the treatment of photometry.

The treatment of thick lenses and combinations of lenses is aided materially by excellent illustrations showing the image resulting from refraction at each of a series of surfaces. It is strange that nodal points are not discussed. This is unfortunate, because, in practice, one usually does not know the curvatures and indices of the components of a compound lens, but can readily locate the nodal points in the laboratory by means of a nodal slide. Following a measurement of the focal length, one can then solve problems with the actual compound lenses one may have on hand. The text omits to explain how this may be done.

It seems to the reviewer that it is an over-simplification to consider the normal magnification to be the highest which will simultaneously give maximum resolution and also normal brightness of the image of an extended object. This is true only if the resolving power of the eye may be considered to be fixed by diffraction alone. For this purpose a pupil diameter of 2 mm is assumed. It is a fact, however, that the pupil diameter may be as large as 7 mm, giving a greater possible retinal illumination while an increase in resolution does not take place because of the aberrations of the eye or the structure of the retina. Furthermore, at low intensities, the retinal structure is effectively coarser because only the receptors having sufficiently high thresholds are in operation. It appears therefore that one should separate the consideration of retinal illumination and resolving power. Then one could point out the difference between the normal magnifying power of a telescope or binocular designed for night-time use as compared with one intended solely for use at high levels of illumination.

The lack of a discontinuity in the reflectivity at the critical angle seems to be overemphasized. A student who has never seen the phenomenon in the laboratory might think that the boundary between partial reflection and total reflection is more difficult to locate than it is in fact.

It should not be overlooked that most of the radiant output from an x-ray tube is in the form of general radiation. The reader of this book gets the impression that the general radiation is a relatively insignificant part of the x-ray spectrum.

Some of the formulas quoted without proof might well have been proved in a simple way, such as the formula for the index of refraction of a prism in terms of the angle of minimum deviation, and the formula for the half-width of the diffraction maxima in the case of a grating. Incidentally, the properly ruled grating does not have opaque regions in the "roughened" spaces as stated. Actually, the grooves are quite smooth furrows, their shape determining the intensity distribution among the various orders.

There are only a few misprints and other errors, some of them so obvious that they do no harm. As pointed out at the beginning, this book has so many good features that it will probably be used quite widely with considerable satisfaction and success.

JOSEPH VALASEK
University of Minnesota

Practical Spectroscopy. GEORGE R. HARRISON, RICHARD C. LORD, AND JOHN R. LOOFBOUROW. Pp. 573+xxxii, Figs. 240, 22×15 cm, Prentice-Hall, Inc., New York, 1948. Price $5.00.

It is probable that the spectrograph is the most versatile instrument in the whole realm of physics. When we consider that it accepts as grist to its mill an almost endless variety of problems extending all the way from the interior

of the atom to the stars, its claim to this distinction would seem to be incontestable.

We who began the study of spectroscopy twenty-five or more years ago well remember the rather erudite and solitary nature of its pursuits then. To the uninitiated, spectroscopy seemed a strange, impractical quest for which they entertained a wholesome respect but little enthusiasm. Probably we shall never recover completely from the astonishment we feel today to discover that our specialty has grown gregarious. Great blast furnaces are kept waiting while the spectrograph completes its ritual. It concerns itself with things as diverse as possible metallic contamination of the baby's canned milk and the tracking of criminals. Industry uses it for innumerable control operations.

As a result, dozens or possibly hundreds of people are deeply interested in some aspect of spectroscopy today where one was before. It seems very good to find that so many are now going our way. This is true not only because man is by nature a social animal. It has very practical advantages as well. That dozens of spectrographs are in use now where one was before makes much commercially available today that could formerly be obtained only by those who could have such things custom built in a precision instrument shop. The basic design, for instance, of the Wadsworth mounting was available for many years before the demand for grating spectrographs of high dispersion became great enough to make its manufacture commercially practical.

George R. Harrison, Richard C. Lord, and John R. Loofbourow have written a book, *Practical Spectroscopy*, that will fill a real need. Written by a physicist, a chemist and a biophysicist, it represents a synthesis of viewpoint that will make it especially valuable as a text and reference book in such a vast field. Ideally, such a book should be based upon exceptional scholarship and extensive experience, as is the case here. The difficulty is that those who are best qualified to write such books so often lack either the time or inclination to assume the burden of doing so.

This is a book that can be read with sufficient comprehension by the beginner in spectroscopy to maintain his interest. At the same time, the more advanced worker will find its content stimulating to read over and over while even those with a lifetime of experience in these fields will find much in it to provoke thought. The reviewer found particularly satisfying the appraisals of various methods with the accompanying hints about their values and limitations. These almost give one the impression that he has had the advantage of a personal conference with the authors. The bibliography is likely to be more than ordinarily useful for it cites references both to put the various subjects into historical perspective and to give the most authoritative recent work. Quite evidently, they have been carefully selected to serve specific ends.

That the book lives up to its title, *Practical Spectroscopy*, is indicated by the fact that there are chapters on the selection of spectroscopic instruments, the testing, adjustment and care of spectrographic equipment, illumination of the spectroscope, photography of the spectrum, light sources, and the identification of spectrum lines. The *why* of the subject is not neglected, however, by undue emphasis upon the *how* for there are also chapters on atomic and molecular spectra. Doubtless those on the measurement of spectral intensities, photographic photometry, qualitative and quantitative spectrochemical analysis will be particularly appreciated by those who are primarily interested in the analytical aspects. Infrared, Raman, vacuum ultraviolet and interferometric spectroscopy also receive attention.

This rather formidable summary of the contents really should not cause the prospective reader to fear a bad case of mental indigestion. The book is too well written for that. Actually, the different chapters represent more or less independent units that will allow him to select those portions that best fit his needs. And, always, the previously unknown chapters may prove a call to travel where he has never been before.

To take fields so vast and give a treatment that is really comprehensive and yet does not bog down from mere multiplicity of detail is a great achievement. The reviewer predicts that there will be many readers who will want to rise and call the authors blessed for what they have done in this book.

KATHERINE CHAMBERLAIN
Wayne University

Principles of Mathematical Physics. WILLIAM V. HOUSTON. Pp. 363+xii, Figs. 42, 6×9 in. McGraw-Hill Book Company, Inc., New York, 1948. Price $5.00.

Many will greet with pleasure this second edition of *Principles of Mathematical Physics* by the new President of Rice Institute. It may be worth mentioning that the author pronounces the first syllable of his name "house" which contrasts nicely with the usual pronunciation of the city of like spelling in which he now lives! Further, one should not confuse this book of McGraw-Hill's International Series with *An Introduction to Mathematical Physics* by R. A. Houstoun, first published in London by Longmans, Green in 1912.

The stated intention of the book is to give the junior, senior, or first-year graduate student "some competence in the techniques of classical mathematical physics and some confidence in his ability to read technical papers in that field." In the beginning of their college work physics students see mathematics and physics nicely separated, taught by separate departments, usually in separate buildings. They soon learn that snatches of, say, calculus are very much in place in their physics work. Then they become ripe for an even closer union of mathematics and physics, in short, for a systematic exploitation of mathematics in their physics. For such as these this book builds a bridge to mathematical physics by alternating, at first, chapters on differential equations with chapters on the mechanics of particles and particle vibrations; then a chapter on the calculus of variations prepares for the next one on Hamilton's Principle. Taken by the hand at first and led across the bridge, the student by this time must learn to put most of his weight on his own feet; the country is getting more rugged, certainly for undergraduates. While the chapter on vibrating systems is limited on the physics side

to the loaded and continuous string it brings on the mathematical side, orthogonal functions, normal modes, and the systematic use of summation indices. A chapter on vectors and another on rigid body dynamics complete a little over half of the 355 text pages in the book. One is tempted to think of this as one semester's work.

Thermodynamics and statistical mechanics are treated next. There follows a separate chapter on vector fields to lay the basis for the following three on electrostatics, magnetism, and the electromagnetic field. In the original edition the vector field was taken up as part of the chapter on vectors: the new arrangement seems pedagogically preferable. The book closes with a sixteen-page chapter on restricted relativity.

As a teacher one cannot escape the prayerful wish that *all* juniors and seniors in physics were such as could handle this material with understanding and competence! To put the point more constructively: it would be very helpful if all textbooks carried, in the preface, a faithful statement of the author's organization of his own course: actual previous preparation required of students, as well as hours per week, number of weeks, number of semesters, and other significant facts based on his teaching experience. If the book merits attention as a text, so do the author's procedures in using it, even though no one would feel compelled to imitate him exactly in all details. It is not at all certain that the sale of a book would be decreased by such pertinent frankness in place of the present prefaces which sometimes seem designed primarily to catch as many customers as possible; if ill-considered or inappropriate use of a text, with students who are not sufficiently prepared or mature, can be prevented a book's reputation will in the long run be enhanced and reorders and financial success may well be greater.

This is certainly no specific criticism of the book under review. Nor, in view of the wealth of material covered, is it a serious criticism to mention the lack of descriptive and experimental background of the physical phenomena. Perhaps the title makes this clear; *theoretical* physics would perhaps connote a greater attempt to orient the reader with respect to observations, phenomena, and laboratory arrangements.

Many teachers will want to supplement the text with discussions of the physics involved. The story is told that Dirac once gave a talk at Leyden. A student asked for further elucidation of one of his points and Dirac replied by repeating verbatim a carefully worded and, no doubt, very correct paragraph of his lecture. With some impatience the venerable Ehrenfest interrupted: "Oh, Dirac! Can't you say it *just a little bit wrong* so ordinary people can understand it?" One can only hope that the story is not completely apocryphal! An oral, qualitative, and physical discussion or a mention of allied questions, applications, and analogies can do much in giving a helpful perspective on the mathematical treatments of this text.

The problems form an integral part of the text and many important results are given in them. However, more examples are worked out by way of illustration in this second edition than was the case in the first, a fact which accounts for much of the 95-page increase in length of the new edition. The practical units of the mks system are used in electricity and magnetism in the new edition although the electrostatic and electromagnetic unit systems are mentioned for the sake of the literature.

One may confidently expect that the new edition will be of even greater value for serious students of mathematical physics than was the first.

RICHARD A. BETH
Western Reserve University

High Resolution Spectroscopy. S. TOLANSKY. Pp. 291+xi, Figs. 114+Plates IV, 14½×22½ cm. Methuen & Company, Ltd., London and Pitman Publishing Corporation, New York, 1947. Price $5.50.

During the past twenty-five years some, at least, of the mysteries of the atomic nucleus have been dispelled. New experimental methods and refined techniques have yielded considerable basic information. One of the most powerful and successful methods has been the study of hyperfine structure of spectra by means of high resolution interferometric techniques. This method forms the subject matter for this book, a book whose importance to workers and teachers in the field of interferometry can hardly be overestimated. It is a unified, authoritative account which contains a wealth of experimental and theoretical material, material which has largely been locked up in relatively few laboratories or has been available only in rather scattered publications,—a book, therefore, which should be widely appreciated by a variety of readers.

A successful study of closely spaced complex spectral lines or hyperfine structure requires an intense sharp-line source for excitation, a dispersive unit or units in combination, capable of high resolution, and finally a device, usually a photographic plate, to record the results for interpretation. Each part is indispensable and progress in the field has depended upon the development and refinement of all. This is recognized at the outset by the author and forms, with minor variations, the order of presentation of topics. First comes a discussion of line breadth since it is often the limiting factor in high resolution work. This is followed by four chapters devoted to light sources, in particular, sharp-line sources. Here one finds an unusually complete discussion of the hollow cathode tube. The atomic beam as a source rates a similar treatment. Eight chapters are devoted to high resolution instruments. All instruments which are currently yielding fruitful results are treated in full detail. Fabry-Perot and Lummer plate interferometers, and transmission and reflection echelons make up the list. Each instrument is presented in a very complete fashion. This includes basic theory, mode of construction, methods of mounting, detailed methods of adjustment, and methods of reduction of the data. Each instrument is discussed carefully as to advantages and limitations. Finally, two chapters are devoted to spectrophotography and the measurement of spectral intensities in high resolution work. This includes an excellent treatment of the photographic process and, an often neglected subject, its influence on the attainable precision in this work. All is presented in a clear, understandable manner, adequately illustrated by a variety of curves,

diagrams, and sketches. All is well documented and although the newcomer to the field will wish to consult other sources in addition for completeness, there are ample references to original papers, reputable texts, and review articles in the field.

This book is of importance not only to research workers in this very elegant branch of optics but to all those who are in any way concerned with interferometry. They will appreciate and find valuable the wealth of experimental detail and the many fine points of technique, a number of which, I suspect, originated in Professor Tolansky's laboratory. College and university teachers of optics will find the treatment of precision interferometry both stimulating and useful. The many practical details will be an aid in the laboratory and the treatment of instruments cannot help but arouse enthusiasm in the classroom.

The book is written in a pleasing, almost informal manner giving one the impression of having had an illuminating discussion with the author and perhaps a personally conducted tour through his laboratory. This manner of presentation has much to commend it when used by a master in the field. The author immediately becomes a teacher with a broader scope for his endeavors. He not only imparts technical information to the student reader, but inspires and challenges him, gives him a sense of acquaintanceship, all of which tend to arouse enthusiasm and increase interest. Professor Tolansky does not neglect this aspect nor yet another. He is careful to point out gaps in our knowledge, the need for further development. Thus he makes numerous suggestions where additional work is desirable. He welcomes newcomers to the field. But let the casual experimenter beware. It is not a simple matter to assemble and adjust apparatus for high resolution work and although the rewards are great one can expect many annoying pitfalls.

I am perhaps predjudiced since I must admit to being a teacher who has been constantly attracted to these powerful optical "weapons of research" but to all who share the attraction I recommend this book.

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Michigan State College

AMERICAN JOURNAL OF PHYSICS VOLUME 17, NUMBER 6 SEPTEMBER, 1949

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AMERICAN JOURNAL OF PHYSICS VOLUME 17, NUMBER 6 SEPTEMBER, 1949

LETTERS TO THE EDITOR

On Jacobian Methods in Thermodynamics

THIS note is to call attention to an unfortunate error in footnote 7, page 4 of an article of the above title appearing in this Journal (Volume **17**, pp. 1–5, January 1949). Instead of $bV=\beta_p$ and $cV=-K_T$ (where β_p and K_T are, respectively, the coefficients of volume expansion at constant pressure and the isothermal coefficient of compressibility) the correct reading should be $b/V=\beta_p$ and $c/V=-K_T$. The writer is grateful to Professor James E. Young for kindly calling the error to his attention.

F. H. Crawford

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Maxwell's Thermodynamic Relations

LETTERS to the Editor having the above title, by Charles M. Focken[1] and H. C. Brinkman,[2] have recently appeared in this Journal. Brinkman's mnemonic scheme is not entirely easy to remember in spite of the alphabetical order of the top line of symbols. It can be argued, of course, that a student who is unable to reconstruct it is not likely to be able to do much of anything with it anyhow. I suggest, however, an alternative. One can take advantage of the cyclic character of the diagram and rearrange it as follows:

$$\begin{array}{ccccccccccccccccc} & & G & & & & H & & & & E & & & & F & & \\ T & / & & \backslash & P & / & & \backslash & S & / & & \backslash & V & / & & \backslash & T \end{array}$$

This can now be remembered by the second-order mnemonic, "The good physicist has studied everything valuable from thermodynamics." I apologize for the "from" and hope that this will be permitted as proper thermodynamic license. The signs can be put in correctly if one remembers that the + sign is associated with the terms containing the differentials of p and S which come first in the alphabet and the − sign with T and V which follow them.

From some points of view this is all pretty trivial but if a little foolishness makes it easy for a student to remember the whole mathematical skeleton of thermodynamics, it may make mental energy available for useful purposes.

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[1] Charles M. Focken, *Am. J. Physics* **16**, 450 (1948).
[2] H. C. Brinkman, *Am. J. Physics* **17**, 170 (1949).

The Electrostatic Behavior of Soap Bubbles

WHILE pursuing a theoretical investigation as to what would happen to a soap bubble if given an electric charge, I was moved to look into the matter experimentally. Accordingly, I equipped myself with a bottle of the bubble liquid which children buy in the 5 and 10, the usual electrostatic paraphernalia, a Zeleny electroscope, and an illuminator for shadow projection. The mechanism for blowing bubbles consists in dipping a metal ring into the solution, withdrawing it, and blowing through it. Invariably a host of bubbles is formed, of all sizes, many of which float earthward for an appreciable time. Some

indeed have a very long half-life! These are caught as quickly as possible, one at a time, by easing them down upon the ring (held by an insulated handle), and their shadow dimension instantly observed. With much speed a charged rod is brought into contact with the metal ring whereby the bubble takes on a charge by conduction. What changes, if any, would *you* expect the bubble to undergo?

A second inquiry arose accidentally. A large stream of bubbles was blown past the charged electroscope from such an appropriate distance that a goodly number burst while within close proximity to the knob. A very substantial flicker of the leaf was observed, sometimes indicating one charge, sometimes the other. What shall we say about this?

Julius Sumner Miller

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Another Solution of the e/m Experiment

PROFESSORS McCombs and Pietenpol[1] and Connell[2] have recently reported on their choice of apparatus for performing the e/m experiment by the Busch and modified Hoag methods. The problem has also been encountered at this institution and the solution has been the assembling of a cathode-ray oscilloscope using a war surplus 5BP1A cathode-ray tube, and the construction of a solenoid for obtaining the magnetic field. The parts for the oscilloscope and the solenoid cost approximately $53.00. The coil is 60 in. long and six in diameter. It has 871 turns of No. 14 insulated copper wire.

The method of performing this experiment is adapted from Hoag's[3] book. The distance from the plate to the screen is obtained from x-ray photographs. The usual controls of the oscilloscope are used for focusing, intensity regulation, centering, etc. The accelerating potential, which may be varied by placing a Variac-type transformer on the input of the oscilloscope,[2] is measured by a vacuum tube voltmeter.

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[1] K. McCombs and W. Pietenpol, *Am. J. Physics* **17**, 78 (1949).
[2] L. F. Connell, Jr., *Am. J. Physics* **17**, 222 (1949).
[3] J. B. Hoag, *Electron and Nuclear Physics* (D. Van Nostrand, ed. 3), pp. 38–41.

American Prose

ALTHOUGH a perfectly good American, I have much sympathy with many of Professor Satterly's complaints[1] concerning current trends in the American language, especially in its pronunciation and meaning of words. We Americans are surprisingly uninterested (the newspapers now say "disinterested") in the fine points of language. As long as we are understood, nothing else seems to matter. But how long shall we remain understood by other English-speaking peoples if we throw overboard all respect for our great common heritage, the language of Shakespeare and Milton? Eccentricities in spelling (though I too abhor "fosforus") do not seem to me as serious as wrong pronunciation and the shift of meaning, as when *disinterested* loses its useful significance of *free from ulterior motives* to mean simply *not interested.* The current use of unique to mean *unusual* instead of *standing alone* is another illustration of a lost value due to sheer ignorance.

As to unnecessary changes in pronunciation Mr. Satterly has pointed out several examples, and I should like to add a few to his list. The most remarkable of these is the tendency in America to shorten long vowels. This is noted by H. L. Mencken in his latest book on the American language. To a physicist the common pronunciation, *ellectricity*, should be as a red rag to a bull. The initial *e*, from the Greek *eta* is of course long, but few persons, even some physicists, seem to know it. When I was a student of engineering at Columbia, we were told that the man who mends your doorbell may be called an *ellectrician*, but that we were to be *ēlectrical engineers*. Another currently shortened vowel is *o* in *solenoid*, derived from the Greek letter omega, but now pronounced *sollenoid* by the unlettered majority of college students. *Raddio* instead of *raydio* (for radio) is the classic example of this evil which Al Smith once made famous. I have collected a long list of such cases representing shortening of each of the five vowels. Some like *virrus* instead of *vyrus* (for virus), and *gropping* instead of *groaping* (for groping) seem almost incredible, but the astute listener can find many other examples almost as bad.

In a somewhat different class is *decible* for *decibel* which ought to make Alexander Graham Bell turn in his grave. Also *crarse* for *cross*, *arle* for *all*, etc., are particularly objectionable, although fairly common in conservative New England. But to return to usage in the physics classroom, I wish to enter a protest against pronouncing π as if it were *pea*, and ϕ as if it were *phee*. These Greek letters were anglicized so long ago that giving them a would-be hellenic sound is as silly as the trick of the high school freshman who refers to Yulius Kaisar to air his first steps in Latin. However, like most people, I admit using the authorized but inconsistent pronunciation, Phy Bayta Kappa for Φ.B.K.

Words derived from the French, like *centimeter*, have long since acquired a right to English pronunciation, but if you must be Gallic, don't make the *centi* French and the *meter* English as is done by many a learned teacher of physics. French sounds, mixed with English, are a modern development in America, like the current pronunciation of sandwich named for that illustrious gambler, the 4th Earl of Sandwich. Today the *sand* becomes the unspellable French for saint, a wholly foreign sound originating, I suppose, with the boy who peddles *saint*-wiches through the train. This hybrid accent is almost universal today among the young who pick up the pronunciation of other illiterate youngsters in preference to that of their elders. In fact Gresham's law applies to pronunciation and usage as much as it does to currency, and debased language drives pure speech out of circulation.

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[1] J. Satterly, *Am. J. Physics* **17**, 167 (1949).